PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCIV.

PART I.

LONDON,

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PART I.

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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds

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of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council.

THE PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged, for the Year 1803, the Medal on Sir GODFREY COPLEY's Donation, to RICHARD CHENEVIX, Esq. F. R. S. for his various chemical Papers, printed in the Philosophical Transactions.

PHILOSOPHICAL TRANSACTIONS.

I. *The* Bakerian *Lecture. Experiments and Calculations relative to physical Optics. By* Thomas Young, *M. D. F. R. S.*

Read November 24, 1803.

1. EXPERIMENTAL DEMONSTRATION OF THE GENERAL LAW OF THE INTERFERENCE OF LIGHT.

In making some experiments on the fringes of colours accompanying shadows, I have found so simple and so demonstrative a proof of the general law of the interference of two portions of light, which I have already endeavoured to establish, that I think it right to lay before the Royal Society, a short statement of the facts which appear to me so decisive. The proposition on which I mean to insist at present, is simply this, that fringes of colours are produced by the interference of two portions of light; and I think it will not be denied by the most prejudiced, that the assertion is proved by the experiments I am about to relate, which may be repeated with great ease, whenever

the sun shines, and without any other apparatus than is at hand to every one.

Exper. 1. I made a small hole in a window-shutter, and covered it with a piece of thick paper, which I perforated with a fine needle. For greater convenience of observation, I placed a small looking glass without the window-shutter, in such a position as to reflect the sun's light, in a direction nearly horizontal, upon the opposite wall, and to cause the cone of diverging light to pass over a table, on which were several little screens of card-paper. I brought into the sunbeam a slip of card, about one-thirtieth of an inch in breadth, and observed its shadow, either on the wall, or on other cards held at different distances. Besides the fringes of colours on each side of the shadow, the shadow itself was divided by similar parallel fringes, of smaller dimensions, differing in number, according to the distance at which the shadow was observed, but leaving the middle of the shadow always white. Now these fringes were the joint effects of the portions of light passing on each side of the slip of card, and inflected, or rather diffracted, into the shadow. For, a little screen being placed a few inches from the card, so as to receive either edge of the shadow on its margin, all the fringes which had before been observed in the shadow on the wall immediately disappeared, although the light inflected on the other side was allowed to retain its course, and although this light must have undergone any modification that the proximity of the other edge of the slip of card might have been capable of occasioning. When the interposed screen was more remote from the narrow card, it was necessary to plunge it more deeply into the shadow, in order to extinguish the parallel lines; for here the light, diffracted from the edge of the object, had entered further into

the shadow, in its way towards the fringes. Nor was it for want of a sufficient intensity of light, that one of the two portions was incapable of producing the fringes alone; for, when they were both uninterrupted, the lines appeared, even if the intensity was reduced to one-tenth or one-twentieth.

Exper. 2. The crested fringes described by the ingenious and accurate GRIMALDI, afford an elegant variation of the preceding experiment, and an interesting example of a calculation grounded on it. When a shadow is formed by an object which has a rectangular termination, besides the usual external fringes, there are two or three alternations of colours, beginning from the line which bisects the angle, disposed on each side of it, in curves, which are convex towards the bisecting line, and which converge in some degree towards it, as they become more remote from the angular point. These fringes are also the joint effect of the light which is inflected directly towards the shadow, from each of the two outlines of the object. For, if a screen be placed within a few inches of the object, so as to receive only one of the edges of the shadow, the whole of the fringes disappear. If, on the contrary, the rectangular point of the screen be opposed to the point of the shadow, so as barely to receive the angle of the shadow on its extremity, the fringes will remain undisturbed.

II. COMPARISON OF MEASURES, DEDUCED FROM VARIOUS EXPERIMENTS.

If we now proceed to examine the dimensions of the fringes, under different circumstances, we may calculate the differences of the lengths of the paths described by the portions of light, which have thus been proved to be concerned in producing those

fringes; and we shall find, that where the lengths are equal, the light always remains white; but that, where either the brightest light, or the light of any given colour, disappears and reappears, a first, a second, or a third time, the differences of the lengths of the paths of the two portions are in arithmetical progression, as nearly as we can expect experiments of this kind to agree with each other. I shall compare, in this point of view, the measures deduced from several experiments of NEWTON, and from some of my own.

In the eighth and ninth observations of the third book of NEWTON's Optics, some experiments are related, which, together with the third observation, will furnish us with the data necessary for the calculation. Two knives were placed, with their edges meeting at a very acute angle, in a beam of the sun's light, admitted through a small aperture; and the point of concourse of the two first dark lines bordering the shadows of the respective knives, was observed at various distances. The results of six observations are expressed in the first three lines of the first Table. On the supposition that the dark line is produced by the first interference of the light reflected from the edges of the knives, with the light passing in a straight line between them, we may assign, by calculating the difference of the two paths, the interval for the first disappearance of the brightest light, as it is expressed in the fourth line. The second Table contains the results of a similar calculation, from NEWTON's observations on the shadow of a hair; and the third, from some experiments of my own, of the same nature: the second bright line being supposed to correspond to a double interval, the second dark line to a triple interval, and the succeeding lines to depend on a continuation of the progression. The unit of all the Tables is an inch.

TABLE I. *Obs.* 9. N.

Distance of the knives from the aperture	-	-	-	-	-	10f.
Distances of the paper from the knives	1f.	3f.	8f.	32f.	96f.	131f.
Distances between the edges of the knives, opposite to the point of coincence	.012,	.020,	.034,	.057,	.081,	.087.
Interval of disappearance	.0000122,	.0000135,	.0000182,	.0000167,	.0000166,	.0000165.

TABLE II. *Obs.* 3. N.

Breadth of the hair	-	-	-	-	-	$\frac{1}{100}$
Distance of the hair from the aperture	-	-	-	-	-	144.
Distances of the scale from the aperture	-	-	-	150,		252.
(Breadths of the shadow	-	-	-	-	-	$\frac{3}{10}$.)
Breadth between the second pair of bright lines				$\frac{2}{10}$.		$\frac{1}{10}$.
Interval of disappearance, or half the difference of the paths				.0000151,		.0000173.
Breadth between the third pair of bright lines				$\frac{1}{10}$.		$\frac{1}{10}$.
Interval of disappearance, $\frac{1}{2}$ of the difference				.0000130,		.0000149.

TABLE III. *Exper.* 3.

Breadth of the object	-	-	-	-	.434.
Distance of the object from the aperture	-	-	-	-	125.
Distance of the wall from the aperture	-	-	-	-	250.
Distance of the second pair of dark lines from each other	-	-	-	-	1.167.
Interval of disappearance, $\frac{1}{2}$ of the difference	-	-	-	-	.0000149.

Exper. 4.

Breadth of the wire	-	-	-	-	.283.
Distance of the wire from the aperture	-	-	-	-	52.
Distance of the wall from the aperture	-	-	-	-	250.
(Breadth of the shadow, by three measurements	.815,	.826,	or .827;	mean,	.823.)
Distance of the first pair of dark lines	-	1.165,	1.176,	or 1.161;	mean, 1.167.
Interval of disappearance					.0000154.
Distance of the second pair of dark lines	-	1.402,	1.395,	or 1.400;	mean, 1.399.
Interval of disappearance	-	-	-	-	.0000137.
Distance of the third pair of dark lines	-	1.594,	1.580,	or 1.585;	mean, 1.586.
Interval of disappearance	-	-	-	-	.0000128.

It appears, from five of the six observations of the first Table, in which the distance of the shadow was varied from about 3 inches to 11 feet, and the breadth of the fringes was increased in the ratio of 7 to 1, that the difference of the routes constituting the interval of disappearance, varied but one-eleventh at most; and that, in three out of the five, it agreed with the mean, either exactly, or within $\frac{1}{160}$ part. Hence we are warranted in inferring, that the interval appropriate to the extinction of the brightest light, is either accurately or very nearly constant.

But it may be inferred, from a comparison of all the other observations, that when the obliquity of the reflection is very great, some circumstance takes place, which causes the interval thus calculated to be somewhat greater: thus, in the eleventh line of the third Table, it comes out one-sixth greater than the mean of the five already mentioned. On the other hand, the mean of two of NEWTON's experiments and one of mine, is a result about one-fourth less than the former. With respect to the nature of this circumstance, I cannot at present form a decided opinion; but I conjecture that it is a deviation of some of the light concerned, from the rectilinear direction assigned to it, arising either from its natural diffraction, by which the magnitude of the shadow is also enlarged, or from some other unknown cause. If we imagined the shadow of the wire, and the fringes nearest it, to be so contracted that the motion of the light bounding the shadow might be rectilinear, we should thus make a sufficient compensation for this deviation; but it is difficult to point out what precise track of the light would cause it to require this correction.

The mean of the three experiments which appear to have been least affected by this unknown deviation, gives .0000127

for the interval appropriate to the disappearance of the brightest light; and it may be inferred, that if they had been wholly exempted from its effects, the measure would have been somewhat smaller. Now the analogous interval, deduced from the experiments of NEWTON on thin plates, is .0000112, which is about one-eighth less than the former result; and this appears to be a coincidence fully sufficient to authorise us to attribute these two classes of phenomena to the same cause. It is very easily shown, with respect to the colours of thin plates, that each kind of light disappears and reappears, where the differences of the routes of two of its portions are in arithmetical progression; and we have seen, that the same law may be in general inferred from the phenomena of diffracted light, even independently of the analogy.

The distribution of the colours is also so similar in both cases, as to point immediately to a similarity in the causes. In the thirteenth observation of the second part of the first book, NEWTON relates, that the interval of the glasses where the rings appeared in red light, was to the interval where they appeared in violet light, as 14 to 9; and, in the eleventh observation of the third book, that the distances between the fringes, under the same circumstances, were the 22d and 27th of an inch. Hence, deducting the breadth of the hair, and taking the squares, in order to find the relation of the difference of the routes, we have the proportion of 14 to 9¼, which scarcely differs from the proportion observed in the colours of the thin plate.

We may readily determine, from this general principle, the form of the crested fringes of GRIMALDI, already described; for it will appear that, under the circumstances of the experiment related, the points in which the differences of the lengths of the

paths described by the two portions of light are equal to a constant quantity, and in which, therefore, the same kinds of light ought to appear or disappear, are always found in equilateral hyperbolas, of which the axes coincide with the outlines of the shadow, and the asymptotes nearly with the diagonal line. Such, therefore, must be the direction of the fringes; and this conclusion agrees perfectly with the observation. But it must be remarked, that the parts near the outlines of the shadow, are so much shaded off, as to render the character of the curve somewhat less decidedly marked where it approaches to its axis. These fringes have a slight resemblance to the hyperbolic fringes observed by NEWTON; but the analogy is only distant.

III. APPLICATION TO THE SUPERNUMERARY RAINBOWS.

The repetitions of colours sometimes observed within the common rainbow, and described in the Philosophical Transactions, by Dr. LANGWITH and Mr. DAVAL, admit also a very easy and complete explanation from the same principles. Dr. PEMBERTON has attempted to point out an analogy between these colours and those of thin plates; but the irregular reflection from the posterior surface of the drop, to which alone he attributes the appearance, must be far too weak to produce any visible effects. In order to understand the phenomenon, we have only to attend to the two portions of light which are exhibited in the common diagrams explanatory of the rainbow, regularly reflected from the posterior surface of the drop, and crossing each other in various directions, till, at the angle of the greatest deviation, they coincide with each other, so as to produce, by the greater intensity of this redoubled light, the common rainbow of 41 degrees. Other parts of these two portions will quit the drop

in directions parallel to each other; and these would exhibit a continued diffusion of fainter light, for 25° within the bright termination which forms the rainbow, but for the general law of interference, which, as in other similar cases, divides the light into concentric rings; the magnitude of these rings depending on that of the drop, according to the difference of time occupied in the passage of the two portions, which thus proceed in parallel directions to the spectator's eye, after having been differently refracted and reflected within the drop. This difference varies at first, nearly as the square of the angular distance from the primitive rainbow: and, if the first additional red be at the distance of 2° from the red of the rainbow, so as to interfere a little with the primitive violet, the fourth additional red will be at a distance of nearly 2° more; and the intermediate colours will occupy a space nearly equal to the original rainbow. In order to produce this effect, the drops must be about $\frac{1}{76}$ of an inch, or .013, in diameter: it would be sufficient if they were between $\frac{1}{70}$ and $\frac{1}{80}$. The reason that such supernumerary colours are not often seen, must be, that it does not often happen that drops so nearly equal are found together: but, that this may sometimes happen, is not in itself at all improbable: we measure even medicines by dropping them from a phial, and it may easily be conceived that the drops formed by natural operations may sometimes be as uniform as any that can be produced by art. How accurately this theory coincides with the observation, may best be determined from Dr. LANGWITH's own words.

" August the 21st, 1722, about half an hour past five in the " evening, weather temperate, wind at north-east, the appearance " was as follows. The colours of the primary rainbow were as " usual, only the purple very much inclining to red, and well

" defined: under this was an arch of green, the upper part of
" which inclined to a bright yellow, the lower to a more dusky
" green: under this were alternately two arches of reddish
" purple, and two of green: under all, a faint appearance of
" another arch of purple, which vanished and returned several
" times so quick, that we could not readily fix our eyes upon it.
" Thus the order of the colours was, I. Red, orange-colour, yel-
" low, green, light blue, deep blue, purple. II. Light green, dark
" green, purple. III. Green, purple. IV. Green, faint vanishing
" purple. You see we had here four orders of colours, and per-
" haps the beginning of a fifth: for I make no question but that
" what I call the purple, is a mixture of the purple of each of
" the upper series with the red of the next below it, and the
" green a mixture of the intermediate colours. I send you not
" this account barely upon the credit of my own eyes; for there
" was a clergyman and four other gentlemen in company,
" whom I desired to view the colours attentively, who all
" agreed, that they appeared in the manner that I have now de-
" scribed. There are two things which well deserve to be taken
" notice of, as they may perhaps direct us, in some measure, to
" the solution of this curious phenomenon. The first is, that the
" breadth of the first series so far exceeded that of any of the
" rest, that, as near as I could judge, it was equal to them all
" taken together. The second is, that I have never observed
" these inner orders of colours in the lower parts of the rainbow,
" though they have often been incomparably more vivid than
" the upper parts, under which the colours have appeared. I
" have taken notice of this so very often, that I can hardly look
" upon it to be accidental; and, if it should prove true in general,
" it will bring the disquisition into a narrow compass; for it will

"show that this effect depends upon some property which the "drops retain, whilst they are in the upper part of the air, but "lose as they come lower, and are more mixed with one ano-"ther." Phil. Trans. Vol. XXXII. p. 243.

From a consideration of the nature of the secondary rainbow, of 54°, it may be inferred, that if any such supernumerary colours were seen attending this rainbow, they would necessarily be external to it, instead of internal. The circles sometimes seen encompassing the observer's shadow in a mist, are perhaps more nearly related to the common colours of thin plates as seen by reflection.

IV. ARGUMENTATIVE INFERENCE RESPECTING THE NATURE OF LIGHT.

The experiment of GRIMALDI, on the crested fringes within the shadow, together with several others of his observations, equally important, has been left unnoticed by NEWTON. Those who are attached to the NEWTONIAN theory of light, or to the hypotheses of modern opticians, founded on views still less enlarged, would do well to endeavour to imagine any thing like an explanation of these experiments, derived from their own doctrines; and, if they fail in the attempt, to refrain at least from idle declamation against a system which is founded on the accuracy of its application to all these facts, and to a thousand others of a similar nature.

From the experiments and calculations which have been premised, we may be allowed to infer, that homogeneous light, at certain equal distances in the direction of its motion, is possessed of opposite qualities, capable of neutralising or destroying each

other, and of extinguishing the light, where they happen to be united; that these qualities succeed each other alternately in successive concentric superficies, at distances which are constant for the same light, passing through the same medium. From the agreement of the measures, and from the similarity of the phenomena, we may conclude, that these intervals are the same as are concerned in the production of the colours of thin plates; but these are shown, by the experiments of NEWTON, to be the smaller, the denser the medium; and, since it may be presumed that their number must necessarily remain unaltered in a given quantity of light, it follows of course, that light moves more slowly in a denser, than in a rarer medium: and this being granted, it must be allowed, that refraction is not the effect of an attractive force directed to a denser medium. The advocates for the projectile hypothesis of light, must consider which link in this chain of reasoning they may judge to be the most feeble; for, hitherto, I have advanced in this Paper no general hypothesis whatever. But, since we know that sound diverges in concentric superficies, and that musical sounds consist of opposite qualities, capable of neutralising each other, and succeeding at certain equal intervals, which are different according to the difference of the note, we are fully authorised to conclude, that there must be some strong resemblance between the nature of sound and that of light.

I have not, in the course of these investigations, found any reason to suppose the presence of such an inflecting medium in the neighbourhood of dense substances as I was formerly inclined to attribute to them; and, upon considering the phenomena of the aberration of the stars, I am disposed to believe, that the luminiferous ether pervades the substance of all material

bodies with little or no resistance, as freely perhaps as the wind passes through a grove of trees.

The observations on the effects of diffraction and interference, may perhaps sometimes be applied to a practical purpose, in making us cautious in our conclusions respecting the appearances of minute bodies viewed in a microscope. The shadow of a fibre, however opaque, placed in a pencil of light admitted through a small aperture, is always somewhat less dark in the middle of its breadth than in the parts on each side. A similar effect may also take place, in some degree, with respect to the image on the retina, and impress the sense with an idea of a transparency which has no real existence: and, if a small portion of light be really transmitted through the substance, this may again be destroyed by its interference with the diffracted light, and produce an appearance of partial opacity, instead of uniform semitransparency. Thus, a central dark spot, and a light spot surrounded by a darker circle, may respectively be produced in the images of a semitransparent and an opaque corpuscle; and impress us with an idea of a complication of structure which does not exist. In order to detect the fallacy, we may make two or three fibres cross each other, and view a number of globules contiguous to each other; or we may obtain a still more effectual remedy by changing the magnifying power; and then, if the appearance remain constant in kind and in degree, we may be assured that it truly represents the nature of the substance to be examined. It is natural to inquire whether or no the figures of the globules of blood, delineated by Mr. Hewson in the Phil. Trans. Vol. LXIII. for 1773, might not in some measure have been influenced by a deception of this kind: but, as far as I have hitherto been able to examine the globules, with a lens of one-fiftieth of an inch

focus, I have found them nearly such as Mr. HEWSON has described them.

V. REMARKS ON THE COLOURS OF NATURAL BODIES.

Exper. 5. I have already adduced, in illustration of NEWTON's comparison of the colours of natural bodies with those of thin plates, Dr. WOLLASTON's observations on the blue light of the lower part of a candle, which appears, when viewed through a prism, to be divided into five portions. I have lately observed a similar instance, still more strongly marked, in the light transmitted by the blue glass sold by the opticians. This light is separated by the prism into seven distinct portions, nearly equal in magnitude, but somewhat broader, and less accurately defined, towards the violet end of the spectrum. The first two are red, the third is yellowish green, the fourth green, the fifth blue, the sixth bluish violet, and the seventh violet. This division agrees very nearly with that of the light reflected by a plate of air $\frac{1}{36400}$ of an inch in thickness, corresponding to the 11th series of red and the 18th of violet. A similar plate of a metallic oxide, would perhaps be about $\frac{1}{15000}$ of an inch in thickness. But it must be confessed, that there are strong reasons for believing the colouring particles of natural bodies in general to be incomparably smaller than this; and it is probable that the analogy, suggested by NEWTON, is somewhat less close than he imagined. The light reflected by a plate of air, at any thickness nearly corresponding to the 11th red, appears to the eye to be very nearly white; but, under favourable circumstances, the 11th red and the neighbouring colours may still be distinguished. The light of some kinds of coloured glass is pure red; that of others, red with a little green: some intercept all the light, except the extreme

red and the blue. In the blue light of a candle, expanded by the prism, the portions of each colour appear to be narrower, and the intervening dark spaces wider, than in the analogous spectrum derived from the light reflected from a thin plate. The light of burning alcohol appears to be green and violet only. The pink dye sold in the shops, which is a preparation of the carthamus, affords a good specimen of a yellow green light regularly reflected, and a crimson probably produced by transmission.

VI. EXPERIMENT ON THE DARK RAYS OF RITTER.

Exper. 6. The existence of solar rays accompanying light, more refrangible than the violet rays, and cognisable by their chemical effects, was first ascertained by Mr. RITTER: but Dr. WOLLASTON made the same experiments a very short time afterwards, without having been informed of what had been done on the Continent. These rays appear to extend beyond the violet rays of the prismatic spectrum, through a space nearly equal to that which is occupied by the violet. In order to complete the comparison of their properties with those of visible light, I was desirous of examining the effect of their reflection from a thin plate of air, capable of producing the well known rings of colours. For this purpose, I formed an image of the rings, by means of the solar microscope, with the apparatus which I have described in the Journals of the Royal Institution, and I threw this image on paper dipped in a solution of nitrate of silver, placed at the distance of about nine inches from the microscope. In the course of an hour, portions of three dark rings were very distinctly visible, much smaller than the brightest rings of the coloured image, and coinciding very nearly, in their dimensions, with the

rings of violet light that appeared upon the interposition of violet glass. I thought the dark rings were a little smaller than the violet rings, but the difference was not sufficiently great to be accurately ascertained; it might be as much as $\frac{1}{30}$ or $\frac{1}{40}$ of the diameters, but not greater. It is the less surprising that the difference should be so small, as the dimensions of the coloured rings do not by any means vary at the violet end of the spectrum, so rapidly as at the red end. For performing this experiment with very great accuracy, a heliostate would be necessary, since the motion of the sun causes a slight change in the place of the image; and leather, impregnated with the muriate of silver, would indicate the effect with greater delicacy. The experiment, however, in its present state, is sufficient to complete the analogy of the invisible with the visible rays, and to show that they are equally liable to the general law which is the principal subject of this Paper. If we had thermometers sufficiently delicate, it is probable that we might acquire, by similar means, information still more interesting, with respect to the rays of invisible heat discovered by Dr. HERSCHEL; but at present there is great reason to doubt of the practicability of such an experiment.

II. *Continuation of an Account of a peculiar Arrangement in the Arteries distributed on the Muscles of slow-moving Animals, &c. In a Letter from Mr.* Anthony Carlisle *to* John Symmons, *Esq. F. R. S.*

Read December 8, 1803.

DEAR SIR,

You did me the honour of presenting to the Royal Society, An Account of a Peculiarity in the Distribution of the Arteries sent to the Limbs of slow-moving Animals.* According to my intention expressed in that letter, I have, since that time, endeavoured to collect farther illustrations of the connection between the disposition of the blood-vessels and the actions of muscles. Neither the tribe of ruminating, nor the carnivorous animals, have afforded the evidence which I had expected, from the investigation of their masseter, pterygoid, and temporal muscles; since these are all supplied by the ordinary arborescent arteries. The rete mirabile, in such animals, seems to be a contrivance to restrain that velocity of the blood which their habits and figures would otherwise produce, in its passage to the brain. The circuitous course of all the arteries which supply the human brain, and their confinement in bony passages, is obviously for a similar purpose. Having sought for examples in other directions, the following results have occurred.

In the human body, two small arteries arise from the upper

* See Phil. Trans. for 1800, p. 98.

part of the abdominal aorta, and, in men, descend from thence to the testicles: they are usually but two in number, and are called the spermatic arteries. Through an extent of ten inches, by average computation, these vessels give off comparatively few lateral branches, so that they may be properly considered as the longest arterial cylinders in the body. The spermatic arteries supply the muscles called cremasters, which suspend the testicles, and the fibrils of the dartos, which corrugates the scrotum. The slow actions of these muscles, and their occasional long continuance in a contracted state, are sufficiently known. The next in resemblance are the intercostal arteries; and lastly those of the diaphragm. Now the slowness of the muscular actions in respiration, and the occasional duration of arbitrary actions in these muscles, need only to be mentioned. Whether the peristaltic motions of the alimentary canal be influenced by the circuitous course of some of the arteries, and their numerous junctions, I am unable to determine: but these arteries are distributed differently, in those respects, from the arteries of ordinary muscles.

The iris, in man, and in animals, is furnished with cylindrical arteries, which pierce the posterior part of the globe of the eye, and finally enter that muscle by a circuitous course.* The pupil of the eye contracts slowly, and is occasionally required to continue long in that state.

The instance of an opposite mode of distribution, is to be found in the coronary arteries which supply the heart, a muscle whose actions are more rapid than those of any other; at the same time it is observable, that the coronary arteries are more

* Vide ZINN. *Descriptio anatomica Oculi humani*, Plate 3, Fig. 2, *b b*, and Fig. 1, *m m*.

quickly subdivided than the arteries of muscles generally. The alteration which the blood undergoes during the supply of this muscle only, is worthy of remark, *viz.* that the coronary veins return venous blood apparently as much changed from the arterial state, as if it had passed through the remotest organs of the body.

Any impediment to the accustomed course of the blood flowing through muscles, induces a corresponding diminution in their powers of action. When the principal arterial trunk which supplies the muscles of the leg is obliterated by ligature, for the cure of an aneurism, the leg remains afterwards much weakened in its muscular strength, until the circuitous vessels have again restored a vigorous supply of blood.

Animals with prehensile tails, such as certain monkeys and opossums, have the muscles of their tails supplied by one cylindrical artery; and the length of time they can suspend themselves by their tails is remarkable, notwithstanding the assistance derived from the repetition of coils around the bough of a tree, and occasionally from an elastic bend in the extreme joints of some prehensile tails.

The swimming-bladders of some fishes afford another example of cylindrical arteries supplying the muscular parts of them. (See Plate I. Fig. 1.) That these parts are truly muscular, I have ascertained by their excitation with the GALVANIC metals. The swimming-bladders appear destined to assist the fish in rising or descending in the water, as well as to keep the back upwards when at rest; so that their muscular actions are probably of slow performance, and require to be of long continuance.

The intestinum ileum of the Cavia Aguti has a similar disposition of blood-vessels; from which I was led to consider the

arteries of the alimentary canal in other animals; and, in many of them, this extension of undivided vessels is to be found. See Fig. 2.

Many of the amphibious class of animals are slow in their motions, such as the tortoises, lizards, and toads; but, whether they can also continue their muscular actions longer than in ordinary cases, I do not know. The blood circulates more slowly in the amphibia; and their respiration is not so important to the vital functions. It cannot however be omitted, that many of the serpents, and some lizards, are very agile.

During the state of contraction, as has been often noticed, the muscles of animals with red blood become of a paler colour, and recover their former redness on the subsequent relaxation; it may therefore be affirmed, that muscular fibres are not distended with blood when in the state of contraction, but that the replenishing with blood is to supply some fresh material, which is employed in muscular action. Whatever substance this may be, it is less required for the irritability of the muscles in some animals than in others. Temperature, and the organs of respiration, seem to be intimately connected with these differences; but all illustrations of such points would extend beyond the limits of the present inquiry.

It has been shewn, that slowness of muscular action, and extraordinary duration of the contractions, are frequently united; and that such unusual phenomena in muscles, are accompanied with a peculiar distribution of the arteries which supply them: but, whether the slowness or the duration be the principal end, or whether the equable supply of blood by a set of appropriate arteries, be the only adaptation convenient for the peculiar offices of such muscles, are subjects not easily determined.

I have considered the accompanying figures of the arteries on the swimming-bladder of the tench, as a striking additional illustration of the subject, though they have been often noticed before, and occasionally ill represented. The intestinum ileum of the aguti is also figured, to show the same kind of distribution. The vessels in both having been injected, for the purpose of a clearer representation.

References to the Figures, Plate I.

Fig. 1 represents a side view of the double swimming-bladder of the tench. *(Cyprinus Tinca.)* The arteries on one side of the hinder bladder being drawn as they appear when injected. The upper portion of the bladder is devoid of those kind of vessels, and does not appear to be muscular. In the hinder portion of the swimming-bladder of the barbel, *(Cyprinus Barbus,)* these parallel cylinders wind spirally round the bladder, instead of proceeding straight forward along the sides.

a, The ductus pneumaticus entering the hinder portion of the vesica aerea.

b, The trunk of the artery from whence the bundles of cylinders are distributed.

c, The upper portion of the vesica aerea, which is covered by a thick, white, opaque, tender membrane.

Fig. 2. The intestinum ileum, and part of the cæcum, of the Cavia Aguti; the vessels being distended with quicksilver.

a, a, a, The intestinum ileum.

b, Part of the appendix vermiformis.

c, Part of the cæcum.

d, d, Anastomosing vessels, crossing the cylinders at right angles, and uniting with each of them.

Future researches in natural history seem to promise a more ample, and a clearer view, of the gross arrangements which attend the structure of muscles; a state of knowledge which will lead physiologists to determine between the essential connections, and the appendages of convenience belonging to muscles, and thereby to see more correctly, the immediate phenomena of muscular actions.

I am, &c.

ANT. CARLISLE.

Soho Square,
Nov. 15, 1803.

Fig. 1.

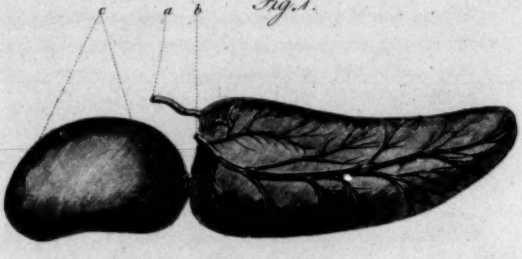
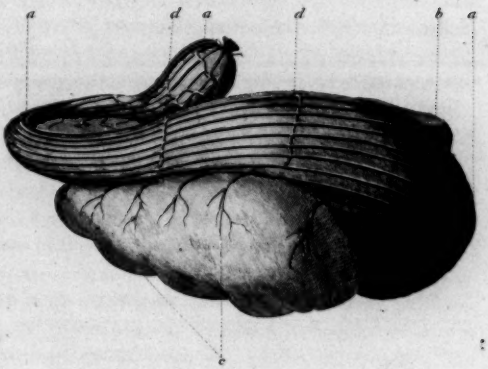


Fig. 2.



Pearse sc.

III. *An Account of a curious Phenomenon observed on the Glaciers of Chamouny; together with some occasional Observations concerning the Propagation of Heat in Fluids.* By Benjamin *Count of* Rumford, *V. P. R. S. Foreign Associate of the National Institute of France, &c. &c.*

Read December 15, 1803.

In an excursion which I made the last summer, in the month of August, to the Glaciers of Chamouny, in company with Professor Pictet of Geneva, I had an opportunity of observing, on what is called the Sea of Ice, *(Mer de Glace,)* a phenomenon very common, as I was told, in those high and cold regions, but which was perfectly new to me, and engaged all my attention. At the surface of a solid mass of ice, of vast thickness and extent, we discovered a pit, perfectly cylindrical, about seven inches in diameter, and more than four feet deep; quite full of water. On examining it on the inside, with a pole, I found that its sides were polished; and that its bottom was hemispherical, and well defined.

This pit was not quite perpendicular to the plane of the horizon, but inclined a little towards the south, as it descended; and, in consequence of this inclination, its mouth or opening, at the surface of the ice, was not circular, but elliptical.

From our guides I learnt, that these cylindrical holes are frequently found on the level parts of the ice; that they are formed during the summer, increasing gradually in depth, as long

as the hot weather continues; but that they are frozen up, and disappear, on the return of winter.

I would ask those who maintain that water is a conductor of heat, how these pits are formed? On a supposition that there is no direct communication of heat between neighbouring particles of that fluid, which happen to be at different degrees of temperature, the phenomenon may easily be explained; but it appears to me to be inexplicable on any other supposition.

The quiescent mass of water, by which the pit remains constantly filled, must necessarily be at the temperature of freezing; for it is surrounded on every side by ice: but the pit goes on to increase in depth, during the whole summer. From whence comes the heat that melts the ice continually at the bottom of the pit? and how does it happen, that this heat acts on the *bottom* of the pit only, and not on its sides?

These curious phenomena may, I think, be explained in the following manner. The warm winds which, in summer, blow over the surface of this column of ice-cold water, must undoubtedly communicate some small degree of heat to those particles of the fluid with which this warm air comes into immediate contact; and the particles of the water at the surface so heated, being rendered specifically heavier than they were before, by this small increase of temperature, sink slowly to the bottom of the pit; where they come into contact with the ice, and communicate to it the heat by which the depth of the pit is continually increased.

This operation is exactly similar to that which took place in one of my experiments, (See my Essay on the Propagation of heat in Fluids, *Experiment* 17,) the results of which, no person, to my knowledge, has yet explained.

There is another very curious natural phenomenon, which I could wish to see explained in a satisfactory manner, by those who still refuse their assent to the opinions I have been led to adopt, respecting the manner in which heat is propagated in fluids. The water at the bottoms of all deep lakes is constantly at the same temperature, (that of 41° FAHRENHEIT,) summer and winter, without any sensible variation. This fact alone appears to me to be quite sufficient to prove, that if there be any immediate communication of heat between neighbouring particles or molecules of water, *de proche en proche*, or from one of them to the other, that communication must be so extremely slow, that we may with safety consider it as having no existence; and it is with this limitation that I beg to be understood, when I speak of fluids as being non-conductors of heat.

In treating of the propagation of heat in fluids, I have hitherto confined myself to the investigation of the simple matter of fact, without venturing to offer any conjectures relative to the causes of the phenomena observed. But the results of recent experiments on the calorific and frigorific radiations of hot and of cold bodies, (an account of which I shall have the honour of laying before the Royal Society in a short time,) have given me some new light respecting the nature of heat, and the mode of its communication; and I have hopes of being able to show *why* all changes of temperature, in *transparent* liquids, must necessarily take place at their surfaces.

I have seen with real pleasure, that several ingenious gentlemen, in London, and in Edinburgh, have undertaken the investigation of the phenomena of the propagation of heat in fluids; and that they have made a number of new and ingenious experiments, with a view to the farther elucidation of that

most interesting subject. If I have hitherto abstained from taking public notice of their observations on the opinion I have advanced on that subject, in my different publications, it was not from any want of respect for those gentlemen that I remained silent, but because I still found it to be quite impossible to explain the results of my own experiments, on any other principles than those which, on the most mature and dispassionate deliberation, I had been induced to adopt; and because my own experiments appeared to me to be quite as conclusive (to say no more of them) as those which were opposed to them; and, lastly, because I considered the principal point in dispute, relative to the passage of heat in fluids, as being so clearly established by the circumstances attending several great operations of nature, that this evidence did not appear to me to be in danger of being invalidated by conclusions drawn from partial and imperfect experiments, and particularly from such as are allowed on all hands to be extremely delicate.

In all our attempts to cause heat to descend in liquids, the heat unavoidably communicated to the sides of the containing vessel, must occasion great uncertainty with respect to the results of the experiment; and, when that vessel is constructed of ice, the flowing down of the water resulting from the thawing of that ice, will cause motions in the liquid, and consequently inaccuracies of still greater moment, as I have found from my own experience; and, when thermometers immersed in a liquid, at a small distance below its surface, acquire heat, in consequence of a hot body being applied to the surface of the liquid, that even is no decisive proof that the heat acquired by the thermometer is communicated by the fluid, from above, downwards, from molecule to molecule, *de proche en proche*; so far from

being so, it is not even a proof that it is from the fluid that the thermometer receives the heat which it acquires; for it is possible, for aught we know to the contrary, that it may be occasioned by the radiation of the hot body placed at the surface of the fluid.

In the experiments of which I have given an account, in my Essay on the Propagation of Heat in Fluids, great masses, many pounds in weight, of boiling hot water, were made to repose for a long time (three hours) on a cake of ice, without melting but a very small portion of it; and, on repeating the experiment with an equal quantity of very cold water, (namely, at the temperature of 41° FAHRENHEIT,) nearly twice as much ice was melted in the same time. In these experiments, the causes of uncertainty above mentioned did not exist: and the results of them were certainly most striking.

The conclusions which naturally flow from those results, have always appeared to me to be so perfectly evident and indisputable, as to stand in no need, either of elucidation, or of farther proof.

If water be a conductor of heat, how did it happen that the heat in the boiling water did not, in three hours, find its way downwards to the cake of ice, on which it reposed, and from which it was separated only by a stratum of cold water, half an inch in thickness?

I wish that gentlemen who refuse their assent to the opinions I have advanced respecting the causes of this curious phenomenon, would give a better explanation of it than that which I have ventured to offer. I could likewise wish that they would inform us how it happens, that the water at the bottoms of all deep lakes remains constantly at the same temperature: and, above all, how the cylindrical pits, above described, are formed

in the immense masses of solid and compact ice which compose the Glaciers of Chamouny?

A remark, which surprised me not a little, has been made by a gentleman of Edinburgh, (Dr. THOMSON,) on the experiments I contrived, to render visible the currents into which liquids are thrown on a sudden application of heat, or of cold. He conceives, that the motions observed in my experiments, among the small pieces of amber which were suspended in a weak solution of potash in water, were no proof of currents existing in that liquid; as they might, in his opinion, have been occasioned by a change of specific gravity in the amber, or by air attached to it. I am sorry that so mean an opinion of my accuracy as an observer should have been entertained, as to imagine that I could have been so easily deceived. For nothing surely is easier, than to distinguish the motion of a solid suspended in a liquid of the same specific gravity, which is carried along by a current in the liquid, from that of a body which descends, or ascends, in the liquid, in consequence of its relative weight, or levity. In the one case, the motion is uniform; in the other, it is accelerated. In a current, the body may be carried forward in all directions, and even in curved lines; but, when it falls in a quiescent fluid, by the action of gravity, or rises, in consequence of its being specifically lighter than the fluid, it must necessarily move in a vertical direction.

The fact is, that I very often observed, in the course of my numerous experiments, the motions of small particles of matter, of different kinds, in water, which Dr. THOMSON describes; but, so far from inferring *from them* the existence of currents in that fluid, their cause was so perfectly evident, that I did not even think it necessary to make any mention of them.

I cannot conclude this Paper, without requesting that the Royal Society would excuse the liberty I have taken in troubling them with these remarks. Very desirous of avoiding every species of altercation, I have hitherto cautiously abstained from engaging in literary disputes; and I shall most certainly endeavour to avoid them in future.

I am responsible to the public for the accuracy of the accounts which I have published of my experiments; but it cannot reasonably be expected, that I should answer all the objections that may be made to the conclusions which I have drawn from them. It will however, at all times, afford me real satisfaction to see my opinions examined, and my mistakes corrected; for my first and most earnest wish is, to contribute to the advancement of useful knowledge.

IV. *Description of a triple Sulphuret, of Lead, Antimony, and Copper, from Cornwall; with some Observations upon the various Modes of Attraction which influence the Formation of mineral Substances, and upon the different Kinds of Sulphuret of Copper.* By the Count de Bournon, *F. R. S. and L. S.*

Read December 22, 1803.

SULPHURET OF LEAD, ANTIMONY, AND COPPER.

This substance, which has hitherto been found only in Cornwall, has been many years known in England. Most of the collections of minerals in London contain specimens of it, in which its peculiar characters are more or less distinctly seen; nevertheless, either the scarcity of this mineral (the mine* in in which it is found having never been regularly worked) has been an obstacle to its being more known in Europe, or the nature of its external characters, which do not allow it to be classed with any of the mineral substances already described, has prevented mineralogists from giving it a place in their works; for, it is very certain that none of them, not even those who have lately written, have taken any notice of it.

In the country where this mineral is produced, it has been generally considered as an antimonial ore; but no observation, at least none that has ever come to our knowledge, had yet determined, whether it was an ore of antimony, properly so called,

* Huel Boys, in the northern part of Cornwall.

or one of those compound metallic bodies in which various metals are combined with the same mineralizing substance, as is frequently observed in mineralogy. At length Mr. HATCHETT, to whom the science of mineralogy is already under many obligations, has added to them, that of having determined, in the most satisfactory manner, the place this substance ought to occupy amongst metallic bodies. Mr. HATCHETT has ascertained, that it is a triple sulphuret, in which the sulphur is combined, at the same time, with lead, antimony, and copper. Mineralogy has hitherto furnished very few instances of triple metallic sulphurets, particularly of such as (like that here described) exhibit their characters in as striking a manner to the mineralogist as to the chemist.

The colour of this mineral is a dark gray, inclining to black. It has a very brilliant lustre.

It is very brittle; fragments of it may be easily broken off by means of the nail.

Its hardness is such, that it very easily cuts calcareous spar; but it is not sufficiently hard to scratch fluor spar.

When rubbed pretty strongly on white paper, it leaves on it a faint black mark; but not so readily as lead, or sulphuret of antimony.

It does not, when rubbed, emit any smell.

When grossly powdered, the powder still retains the metallic lustre.

When thrown, in the last mentioned state, upon an iron not quite red hot, it emits a phosphorescent light, of a bluish-white colour, but without any smell whatever; no such light, however, can be obtained from it by means of friction.

Its specific gravity is 5765;* it is consequently superior to that of sulphuret of copper, or sulphuret of antimony, but very inferior to that of sulphuret of lead.

The fractures of its crystals are not smooth, neither are they lamellated in any particular direction, but are generally granulated, and have rather a coarse grain.

The crystals of this substance are very brilliant, and often of a very large size. I have seen some that were more than an inch in length, and of a proportional height: but, as most of them have a great number of facets, and are frequently very irregularly shaped, on account of the inequality of their increase, it becomes very difficult to determine their form; particularly as the crystals most commonly found, are those which differ most from the primitive form, to which, on that account, it becomes very difficult to refer them. For this reason, I have resolved to give a particular description of every thing that relates to the crystalline forms of the substance; hoping that I shall, by that means, contribute to promote the knowledge of a character so important in the study of mineralogy.

Primitive Crystal. The form of the primitive crystal is a rectangular tetraedral prism, which has its terminal faces perpendicular to its axis, as in Plate II. Fig. 1; but I have never yet observed, in all the specimens I have seen of this substance, the above-mentioned prism entirely destitute of secondary facets.

* Of two trials of the specific gravity of this substance, taken at six months distance from each other, the first gave me 5765, the second 5763. It is evident that it is scarcely possible to approach more nearly to that found by Mr. HATCHETT, namely, 5766; and this constancy in the above character, may be considered as a proof of the state of purity habitual to this sulphuret.

*First Modification.** This modification consists in a decrease along the vertical edges of the primitive crystal, so that each of these edges is replaced by a plane, which is equally inclined upon the adjacent faces, and which consequently makes, with each of them, an angle of 135°, as in Fig. 2. Among the crystals I have hitherto seen of this substance, I have never yet met with this simple octaedral prism; it is usually combined with some of the other modifications, and, in that case, the sides of the secondary prism are generally broader than those of the primitive crystal, as is represented in Fig. 3. The new planes produced by this first modification, are usually striated, and often very deeply, in their vertical direction.†

When the new planes abovementioned have acquired sufficient extent to cause the vertical faces of the primitive crystal entirely to disappear, the result is, a rectangular tetraedral prism, perfectly similar to that of the primitive crystal, excepting that, in all I have met with, I have constantly observed that the vertical faces were striated, as in Fig. 4. This kind of prism is rare in this substance; I have, however, met with two or three instances

* I commonly make use of the term *modification*, to express that alteration which takes place in the primitive crystal, whenever the crystalline laminæ (which are themselves only a regular collection of molecules) undergo a change with respect to the ratio of the arithmetical progression they admit, in their deviation from the edges or angles of the primitive crystal; and I employ the word *variety*, to signify those differences in the crystals which arise only from the various proportions of the faces to each other, or those which are produced by the combination of two or more modifications in the same crystal.

† Although, in all these crystals, the above-mentioned striæ are owing to an imperfection in their crystallization, I have, in the figures, represented by strokes, those striæ which distinguish the planes of the secondary prism of this substance. By this means, it becomes more easy to comprehend the manner in which the crystals differ from each other.

of it. The above-mentioned vertical striæ are sometimes so strongly marked, as to give reason to suppose we might, by a very trifling effort, succeed in separating the laminæ from each other, according to their natural direction; but every attempt I have made with this intention, has constantly been fruitless.

Second Modification. This modification is produced by a decrease at the solid angles of the primitive crystal, in consequence of which, each of these angles is replaced by a plane, which makes, at its meeting with the terminal faces, an angle of 130°; and, with the vertical edges of the prism, an angle of 140°, as in Fig. 5. I have never yet observed the planes produced by this modification, simply combined (as is shown in the figure) with those of the primitive crystal; but the crystals I shall now describe, will demonstrate the probability of the existence of such a combination. I have, however, seen these planes combined with those of the secondary prism; and, when that happens, the planes caused by this second modification, instead of being situated at the solid angles of the prism, (as is the case when they are combined with the planes of the primitive crystal,) are situated on the edges of the terminal faces.

Third Modification. This modification is also produced by a decrease at the solid angles of the primitive crystal; but this decrease is greater than the former, that is to say, the edges of the laminæ, which are deposited one upon the other, recede in a greater degree from the edges of those which have preceded them. This gives rise to a plane, which, in a similar manner, replaces the solid angles of the primitive crystal, and makes, at its meeting with the terminal faces, an angle of 150°; and, with the edges of the prism, one of 120°. See Fig. 6. I have not indeed hitherto seen the planes produced by this third modification

combined merely with those of the primitive crystal; but it is yet more common than in the preceding modification, to meet with them combined with the planes of the secondary prism; and they are then situated along the edges of the terminal faces, as is shown in Fig. 7. More frequently, however, the prism is very short, as is represented in Fig. 8; and it usually preserves some traces, more or less considerable, of the sides of the primitive crystal, as in Fig. 9.

It sometimes happens, that the decrease producing this modification has taken place only in two of the opposite angles, situated in the same direction, on the terminal faces; then, if the planes arising from this modification happen to be combined with those of the secondary prism, they are situated only on two opposite edges of each of the terminal faces of this prism. See Fig. 10. I have observed this variety in pretty large crystals; and the specimen in which I met with them (and which, like all the others, came from Cornwall) contains also some very regular aggregations, each consisting of four of these crystals, in the form of a cross. These crystals penetrate each other for a certain part of their extent, as is shown in Fig. 11.

If the planes produced by this third modification were combined with those arising from the second modification, the result would be, the variety represented in Fig. 12, in which the planes of the two last modifications meet together at an angle of 170°; but I have not yet met with this variety in so simple a state.

Fourth Modification. This is produced by a decrease along the edges of the terminal faces of the primitive crystal; in consequence of which, each of those edges is replaced by a plane, which is equally inclined upon the adjacent faces, and makes,

with the terminal faces, and with the vertical faces of the prism, an angle of 135°, as in Fig. 13.

If the planes produced by this fourth modification were combined with those of the secondary prism, they would then be situated at the solid angles of this prism, as is represented in Fig. 14. I have not yet seen these two last varieties in so simple a state as that in which the figures represent them; but the two most common forms in which this substance has hitherto been found, are those shewn in Figs. 15 and 16. Now, Fig. 15 is nothing more than the combination of the planes of the primitive crystal with those of this modification, and with those of the first and second modifications. In the same way, Fig. 16 is likewise the result of the same combination; but with the planes of the third modification added to it.

Lastly, Fig. 17 exhibits a detached crystal, of a perfectly determinate form, and of about an inch in length, seven lines in breadth, and five lines in height, which makes part of the collection of Mr. PHILLIPS, who was so kind as to allow me to examine it, as well as every other specimen of this substance in his possession. It is, in fact, the variety represented in Fig. 16, but in which the planes belonging to the fourth modification have acquired a more considerable extent; while those which belong to the third modification exist only on two of the opposite edges of the terminal faces, as was observed in speaking of the crystal represented in Fig. 10.

Before I proceed to those observations which this triple sulphuret (a most interesting substance in mineralogy) affords me an opportunity of making, with regard to the various ores that are produced by the combination of sulphur and copper, of the nature of which, neither mineralogy nor chemistry have yet

supplied any certain information, I think it right to make a few remarks, upon some circumstances relating to the different modes of attraction that appear to influence the formation of mineral substances. However imperfect this sketch may be, it will at least serve to illustrate what I shall hereafter say, respecting the various ores I have just mentioned.

OBSERVATIONS ON THE VARIOUS MODES OF ATTRACTION WHICH INFLUENCE THE FORMATION OF MINERAL SUBSTANCES.

The observations to which the study of mineralogy, which of late years has been pursued with particular attention, has given rise, seem to me to lead to the following conclusion, *viz.* that, of the two kinds of attraction which have been hitherto admitted to prevail in the formation of mineral substances, namely, the *attraction of composition*, and the *attraction of aggregation*, the latter is subject to different modes of action, all of which have a striking effect in the formation of mineral substances.

The first kind of attraction to which mineral bodies are subject, and which is generally known by the name of chemical attraction, is the *attraction of composition*. This kind of attraction takes place only between the most simple or primitive molecules of a substance; but, at the same time, it exists only between molecules that are dissimilar, or that belong to different substances. To its action is owing the formation of new molecules, to which may be properly given the name of *secondary* or *integrant* molecules; because they, and they only, determine the nature of all the compound bodies belonging to the mineral kingdom. These molecules are the result of the intimate com-

bination, in different proportions, of the primitive molecules of two or more different substances. The difference existing between mineral bodies, consequently depends upon the following circumstances; first, upon the nature of the primitive molecules, by the combination of which they are produced; secondly, upon the proportion in which those molecules are combined together.

At the instant the new or secondary molecules are formed, if they happen to be at a proper distance from each other, and in a fluid which permits them to move freely, they become subject to, and are forced to obey, the second species of attraction, namely, the attraction of aggregation, which unites them into one or several masses, perfectly homogeneous in all their parts.

But the attraction of aggregation seems to be susceptible of various modifications, which alter its manner of acting upon the constituent molecules. Of these modifications there are two principal ones; the first of which may, I think, be distinguished by the name of *crystalline attraction of aggregation*; the second may be called *simple attraction of aggregation*.

The crystalline attraction of aggregation always takes place between similar molecules; which molecules are simple or primitive, in those bodies which are considered as simple or primitive, (and which in fact are so,) and they are compound in other bodies. This kind of attraction, in its action upon the molecules under its influence, is either *regular*, *irregular*, or *amorphous*.

In the regular crystalline attraction of aggregation, the molecules arrange themselves in such a way as to give rise to solid bodies, which are either constantly of the same form, or are subject to certain laws of variation; these variations, however,

are always capable of being referred to the same primitive form. This kind of attraction, to exert the full action of which it is capable, requires (as well as the attraction of composition) that the molecules on which it acts should be situated in a fluid, in order that those molecules may possess a perfect freedom of motion. It is also necessary that this fluid be at rest; any motion foreign to that of the molecules themselves, disturbs them, and necessarily proves an obstacle to that regularity of form which is the natural result of their exact union. The fluid must, at the same time, be in a situation where it can evaporate gently; which evaporation, causing the molecules slowly to approach each other, brings them successively within their sphere of attraction. A crystalline mass may however show no regular determinate form, and nevertheless be the result of a regular crystallization. Such, for instance, is the formation of the pure transparent massive carbonate of lime, known by the name of calcareous Alabaster, which exactly fills the fissures or cavities in which it is found; such also is the formation of those stalactites which are composed of this kind of alabaster. In this case, as the cavities are completely filled, there cannot be formed in them any distinct crystal; and the above-mentioned masses may be considered as a large aggregation of crystals, the sides of which, being similar in form, and situated in the same direction, adhere together in one indeterminate mass. But, when that happens, the fracture of the mass always shows, in a satisfactory manner, the nature of its formation. In the substances I have just spoken of, for instance, the fractures are always lamellated; and, by following the direction of the lamellæ, we may always bring any fragment of the mass into the form of its primitive crystal.

When, in the formation of a substance that is acted upon by the crystalline attraction, its action happens to be disturbed, by any cause whatever, the masses resulting from it do not belong to the *regular* crystalline attraction; and the *irregular* crystalline attraction which takes place, produces various results, which no doubt are owing to many different causes. Sometimes there are masses partially lamellated, the lamellæ of which cross each other in various directions; at other times, we find masses which are either foliated, or fibrous.

It sometimes happens, (owing perhaps to a more considerable degree of disturbance during the process of attraction,) that there are formed small irregular detached masses, often so minute as to be scarcely perceptible; at other times, they are of a larger size, and, as soon as formed, fall to the bottom of the liquor, and unite together by a simple mode of attraction, which may with great propriety be called *simple homogeneous attraction of aggregation*. Of this kind are, granulated quartz, granulated carbonate of lime, &c. the different kinds of which substances, differ from each other only by the fineness or coarseness of their grain. Sometimes, however, the crystalline attraction of aggregation, and the simple homogeneous attraction, act together, at the same time, in the same solution. When this takes place, the granulated masses, instead of being composed of an aggregate of irregular grains, appear to consist of small crystals, which have a very regular form. This frequently takes place in pure granulated carbonate of lime, and still more frequently in magnesian carbonate of lime, particularly in that kind which is found in large and extensive masses, in various parts of England. In the case here spoken of, which, it may be presumed, can take place only in highly saturated solutions, in which the molecules

are consequently very near to each other, there is a rapid and detached crystallization, in all parts of the solution, while no attraction takes place between the crystals that fall down, except that of simple aggregation. I repeat, that no other than simple attraction of aggregation takes place between the crystals; for, if the crystalline attraction, which first took place, had continued to exert its action, the crystals, instead of joining together in a confused and irregular manner, would unite by their analogous sides, and produce one or several very large crystals; which crystals would be either exactly similar to the primitive crystals, (of which we see such frequent instances in fluate of lime, and in sulphuret of lead,) or they would have secondary planes, produced by a regular decrease, similar to those which have been mentioned and described by others, as being formed only by the molecules of the primitive crystalline form. These decreases can, in this case, only modify the secondary form; in the same way as the decreases produced by the primitive molecules, modify the primitive form. This new law, of which till now no notice has been taken, will perhaps appear at first view to militate against the generally received ideas of crystallization; its existence, however, cannot be doubted. In fluate of lime, and also in carbonate of lime, are frequently seen irrefragable proofs of it; indeed it appears to me that, far from contradicting the laws of attraction hitherto observed in nature, it agrees perfectly well with them.

Sometimes, in the great dissolutions of nature, the molecules, instead of uniting together by the influence of the crystalline attraction of aggregation, are precipitated in a detached but confused manner. In that case, the action of this mode of attraction has entirely ceased, and has given place to that of simple

aggregation; and the masses that result from it, no longer offer any appearance that can recal to the mind the known circumstances of crystallization. The fractures of the mass have no lamellated or regular aspect; and very often present that which is distinguished by the name of *earthy*. Such is, in various substances, the formation of most of those varieties to which Mr. WERNER has given the name of *compact*.

Many circumstances seem to lead to the idea that, in the formation of certain substances, there exists a species of crystallization by which no determinate form is produced. These substances, however, are really the result of a regular crystallization, that is to say, of as regular a crystallization as can take place in them, and one which cannot be referred to any of the disturbed crystallizations of which I have already spoken. This property depends, probably, upon the peculiar form of the primitive molecules of those substances; such, for instance, is the globular form, or those forms which approach to it: perhaps, however, it depends upon other causes. This species of attraction, is what I have called *amorphous attraction of crystallization*; a name that, in my opinion, expresses very properly the nature of its action, which appears not to allow the substances on which it acts, to take any determinate geometric form, however pure those substances may be, or under whatever circumstances that action may take place. Calcedony, girasol,* (which substance, when in a certain state of decay, is called opal,) and that kind of steatite which is perfectly pure and transparent, appear to me to owe their origin to this mode of formation.

* I have long since adopted this word, (which had already been given to one of the purest varieties of the above substance,) to distinguish the substance to which Mr. WERNER gives the general name of opal, and to which the Abbé HAUY gives the name of *quartz resinite*.

This subject requires that I should enter into it more minutely. In so doing, I shall make calcedony the basis of my observations, which may be afterwards easily applied to such other substances as are similarly circumstanced. I shall first observe, that calcedony, by the purity and homogeneity it is frequently seen to possess, as well as by its nature and the grain of its fracture, positively announces a state of crystallization; yet, although we frequently observe it in circumstances exactly similar to those which, in other substances, give rise to determinate crystals, (such as actual solution in a fluid, and that fluid inclosed in a cavity where it is in a state of rest,) it always offers itself to us with the same appearance, in which there cannot be perceived any tendency to a regular form. Every mineralogist is acquainted with the beautiful geodes of Oberstein, in the cavities of which are seen crystals of quartz, also of carbonate of lime, and very frequently, likewise, fine crystals of the substance called by Werner *Kreutzstein*, and by the Abbé Haüy *Harmotome*. Calcedony is frequently found with the above substances; but, when that happens, although it is very pure, and is in the same circumstances as these substances, it always appears either in the form of layers, or of mamillæ, or in the ramose form of stalactites; and never shows the least tendency to a determinate crystalline form. The same remark may be applied to other geodes, composed of agate, of flint, &c. Indeed, if calcedony were capable of assuming a determinate crystalline form, how does it happen that this substance, (which is one of the most common in nature, and is found in so many different forms, and so variously circumstanced,) has never yet been met with in a state that shewed any appearance of that form. It is true, that many mineralogists have spoken of crystals of calce-

dony; but, from the variety of forms they have assigned to these crystals, (which forms have no connection with each other, and evidently belong to other known substances,) it is very clear that all these pretended crystals of calcedony, have been produced merely by its having taken the place of other substances; either by being moulded in the cavities left by the destruction of the crystals of such substances, or by being formed at the very instant of the destruction of those crystals, by an operation analogous to that known by the name of cementation. Examples of the nature here spoken of, are well known to happen in many other substances. Besides, those stones which offer the greatest resistance to a mechanical division, such as quartz, blue corundum or sapphire, zircon, garnet, &c. sometimes show, by fortunate accidental fractures, or other natural accidents, the direction of the crystalline laminæ of which they are composed. Quartz, for instance, is sometimes found with an evidently laminated appearance, particularly the blue variety, which is brought to us either from Canada or from the East Indies; but never yet was an appearance of this nature perceived in any kind of calcedony.

Many celebrated mineralogists, amongst whom may be mentioned the Abbé Hauy, seem to explain this want of crystallization in calcedony, by considering that substance as nothing more than quartz in a concrete state. This is, in fact, supposing it a sort of irregular crystallization, which bears the same analogy to quartz, that calcareous alabaster bears to crystallized carbonate of lime. I cannot, however, by any means adopt this opinion, as it does not appear to me to agree with those facts which nature permits us to observe; for, not to mention those kinds of quartz which are produced by a disturbed crystallization,

for instance, granulated quartz, both of a fine and of a coarse grain, quartz in stalactites, &c. we are acquainted with several kinds of true concrete quartz, which forms veins in primitive rocks, in the same manner as carbonate of lime forms veins in secondary rocks. All these substances have the appearance peculiar to quartz, and show nothing that is at all analogous to that appearance which so strongly characterises calcedony: the latter, also, possesses a degree of hardness superior to that of any of the others. Besides, quartz in a concrete state is often found accidentally mixed with martial argill; a circumstance that, as is well known, frequently happens to calcedony. In the former case, a true jasper is formed, which has a quartzose base, instead of having, as in the latter case, a base of calcedony. The appearance of these two kinds of jasper is so different, that the most inexperienced eye cannot fail to distinguish them at first sight. Many other facts might be added, in support of what I have said. Calcedony, for example, is easily decomposed: there are few masses of this substance of any considerable size, few fragments of flint, jasper, &c. which have lain for any length of time upon the surface of the earth, that do not afford a demonstration of it. Most kinds of cacholong are nothing more than the effect of this decomposition, in a more or less advanced state, which (as happens in the girasol, but in a less perfect and less striking manner) causes the stones in which it takes place, to possess the property of becoming transparent in water. Quartz, whether in the most perfectly crystallized state, or in that in which its crystallization has been the most disturbed, possesses nothing which can be compared with the above property. I cannot therefore consider quartz as calcedony, properly so called; nor can I consider the substance I have distinguished

by the name of girasol, as being either quartz or calcedony. But, it will be said, what is then the nature of those substances? To this question I can only answer, I do not know; and, unfortunately, those substances are not the only ones respecting which we are obliged to confess our ignorance. We make, however, one step towards the knowledge of the nature of a substance, when we discover that it is not what it has hitherto been supposed to be. Calcedony and girasol certainly have for their base, the same earth that forms the base of quartz; but that earth appears to me to be differently modified in those substances; that is to say, I do not believe that quartz is merely an aggregate of pure quartzose earth. In like manner, I do not believe that corundum is merely an aggregate of pure argill. Perhaps, among the number of interesting discoveries with which chemistry is constantly enriching us, we shall one day be enabled to place that of a more intimate acquaintance with the nature of the above-mentioned substances.

Besides the attraction which takes place between the *similar* integrant molecules of substances, which I have already mentioned, there exists another, between those similar molecules and those which are *dissimilar*, or of a different nature. In consequence of this kind of attraction, during the formation of a substance by the aggregation of its similar molecules, other molecules of a different substance, being situated near them, enter into union with them, either by one molecule uniting with another, or by a collection of molecules uniting themselves to a molecule. To the first of these, I shall give the name of *simple homogeneous attraction of aggregation*; the other I shall call *heterogeneous attraction of aggregation*; and, to those extraneous molecules which the last mentioned kind of attraction introduces

into a substance, I shall give the name of *accidental integrant molecules*.

The effects of the last mentioned attraction, which is much weaker and much more variable than any of the others, seem to depend, in a great measure, upon the different circumstances in which the substances happen to be, at the moment of their formation. The new substance resulting from it, is not perfectly homogeneous in all its parts, and, of course, ceases to be a chemical compound, properly so called. Every thing seems to show, that the introduction of these accidental molecules into the abovementioned substances, is governed by a particular law of attraction; which acts in an uniform manner, so long as the formation of the substances under its influence takes place in the same circumstances; but which varies, when those circumstances happen to undergo any variation. Thus, for instance, in those kinds of tremolite that have the dolomite for matrix, (as in that from St. GOTHARD,) the carbonate of lime is in the proportion of $\frac{14}{100}$;* but, in those kinds which have an argillaceous matrix, it is only in the proportion of $\frac{4}{100}$. Thus also, the magnesian carbonate of lime, the chemical nature of which, as is shown by its form, is the same as that of pure carbonate of lime, admits, in consequence of the heterogeneous attraction of aggregation, the magnesian earth as a simple integrant part, and in proportions which vary, according to the circumstances in which the formation of this carbonate takes place. The magnesian carbonate of lime of Tyrol, for example, contains, according to

* Assisted by the analysis of Mr. CHENEVIX, I think I have proved what I have here asserted; and also, that the phosphorescence of the tremolite, which had been considered as one of the characters of that substance, is owing only to the particles of dolomite that are mixed with it. See *Journal des Mines*, No. 73.

Mr. Klaproth, $\frac{4}{100}$ of magnesia;* while, according to the same chemist, that which comes from Sweden, contains only $\frac{2}{100}$; yet both of them, most commonly, assume the primitive rhomboidal form of pure carbonate of lime.

In like manner also, a proportion of iron greater than that which makes a constituent part of the garnet, is observed in that substance; and this proportion varies considerably, according to the circumstances which governed its formation. If we take, from among the various analyses of this substance, by different chemists, those only which were made by Mr. Vauquelin, we shall find, that the red garnet of Bohemia afforded him $\frac{4}{100}$ of iron; the black garnet of the *Pic d'Eres-lids* in the Pyrenees

* This is likewise nearly the proportion of magnesia found in the magnesian carbonate of lime which is so common in Derbyshire, and in many other parts of England. For our knowledge of this substance, which had till then been confounded with the martial carbonate of lime, (pearl spar,) we are indebted to the analysis made by Mr. Tennant. It is found in the form of small but very brilliant crystals, which belong to the primitive rhomboid of pure carbonate of lime; but these rhomboids do not appear to be subject to those variations in form so commonly met with in martial carbonate of lime. More frequently, however, it is in the form of a more or less granulated mass, which very often, upon being examined with a lens, shows itself to be a confused aggregation of the same small rhomboids. This magnesian carbonate of lime, is nearly equal in hardness to the martial carbonate. It dissolves, however, much more readily, and with a slight effervescence, in nitric acid; but does not give the same yellow colour to that acid, when it happens to contain iron, which it almost always does, though in very small proportion. Its mean specific gravity, taken on three specimens from different parts of Derbyshire, which varied very little from each other, was found to be 2823; the same carbonate from Tyrol, gave a mean specific gravity of 3053. The specific gravity of the magnesian carbonate is therefore greater than that of pure carbonate of lime; and there must surely have been some error in the operation that gave to Mr. Klaproth 2480, as the specific gravity of the former substance. The magnesian carbonate shows no phosphorescence when thrown upon a hot iron. I think it probable that, in the cabinets of various mineralogists, specimens of it have been erroneously placed among those of pearl spar.

$\frac{14}{100}$;* the red garnet from the same place $\frac{17}{100}$; and the yellow garnet of Corsica $\frac{19}{100}$.

Nevertheless, although the various molecules which, by means of this last-mentioned mode of attraction, unite themselves to mineral substances during their formation, do not cause any change in their chemical nature, they frequently, as I have already observed, occasion an alteration in their physical construction; and very often induce variations in such of their characters as most immediately depend upon that construction, such as, their specific gravity, their hardness, their transparency, and even (particularly in the class of stones) their colour. It is therefore necessary that the mineralogist should fix his chief attention upon this mode of attraction, in order that he may be able to understand the accidental causes of the variations to which the substances under his examination are subject. The chemist also ought always to bear in mind the existence of such causes, as they may be fairly suspected, in most of the substances whose nature he attempts to investigate by his analysis. If he neglects to do this, he will be constantly liable to confound, in the result of his operations, those products which really belong to the chemical composition of the substances he examines, with those which are foreign to it.

The foreign particles which the heterogeneous attraction of aggregation thus introduces into mineral bodies, necessarily affect their homogeneity. Yet, when that mode of attraction has taken place with all the perfection of which it is susceptible, the

* The black garnet from Frascati near Vesuvius, of which Mr. Werner (for I know not what reason) has made a particular species, was also found by Mr. Klaproth to contain $\frac{16}{100}$ of iron; whereas Mr. Vauquelin found the proportion of iron in this kind of garnet to be as high as $\frac{33}{100}$.

interposition of these particles, in the substances into which they are admitted, is made in such a regular manner, that the homogeneity of those substances is in some measure preserved, if not with respect to single molecules with each other, at least with respect to collections of molecules with similar collections. Hence it follows, that the substance, although it may not possess that complete transparency which belongs to it when in its highest state of chemical purity, still retains that property in a very considerable degree. This is exemplified in many crystals of magnesian carbonate of lime, of martial carbonate of lime, of garnet, &c. But, for the most part, this kind of attraction, which, on account of its being more weak than any of the others, is more easily disturbed, does not admit of the forementioned regularity; in that case, the substance which was under its influence, possesses a greater or less degree of opacity.

In short, it appears, that the molecules of foreign substances, introduced into mineral bodies, in the above-mentioned manner, by the heterogeneous attraction of aggregation, do not prove any obstacle to the action of the crystalline attraction. The only effect the former mode of attraction seems to produce upon the latter, is to cause the form of the substance submitted to its influence, to approach as nearly as possible to the most simple forms, or even to the primitive one, belonging to it; and, at the same time, to render those forms more constant. Thus, the magnesian carbonate of lime, and also the martial carbonate of lime, generally assume either the form of the primitive rhomboid, or that of the lenticular rhomboid, of pure carbonate of lime. Thus also, the quartzose carbonate of lime, commonly known by the name of sandstone of Fontainebleau, constantly assumes the form of the muriatic rhomboid (named by HAUY

inverse) of the pure carbonate of lime. Lastly, in the same manner, the kind of talc called chlorite, which frequently introduces itself into the axinite, almost always occasions the latter substance to assume one of its most simple forms.

Those stones in which there exists no other cause of union between their particles than the attraction of aggregation, and which are known by the name of aggregate stones, furnish an example of the attractive force that is really exerted by the dissimilar molecules which enter into substances during their formation. In granite, for instance, the integrant parts, which, instead of being molecules, are become masses, are as dissimilar as possible. Yet, although no ingredient whatever contributes to unite them, (their union being brought about merely by the cohesion of their surfaces,) the great degree of hardness this stone possesses, and the difficulty with which its parts are separated, when it is in a perfect state, that is to say, when its texture has not been injured by any accidental cause, are both well known. This remark may be applied to various kinds of sandstone, of schistus, &c.

The different kinds of attraction here described, may, I confess, be nothing more than mere modifications of one and the same power, originally belonging to matter; but this appears to me not yet sufficiently demonstrated. Supposing it, however, to be the case, they certainly exert as much force upon mineral substances, at the time of their formation, as could be exerted by attractive forces of a really different nature.

OBSERVATIONS UPON THE DIFFERENT KINDS OF SULPHURET OF COPPER.

The triple sulphuret of lead, antimony, and copper, described by me in the former part of this Paper, cannot fail to be considered as a substance highly interesting both to mineralogists and chemists, as it serves to show the true nature of the triple combination which sulphur enters into, with lead, antimony, and copper. The difference existing in the form, the specific gravity, the hardness, and all the other external characters of this triple sulphuret, when compared with those of the gray tetraedral sulphuret of copper, seems to me to demonstrate, that the antimony and the lead which have been so frequently supposed to be constituent parts of the last-mentioned ore, are nothing more than accidental mixtures; and that, when they happen to be met with in that ore, they have been introduced merely by the heterogeneous attraction of aggregation, and are foreign to its substance. The same opinion may, I think, be fairly adopted, respecting the silver which is sometimes found in it. If these metals were really contituent principles, how can we suppose, that the presence or the absence of one or more of them, and the great difference that exists in their proportions, (as is shown by the various analyses which have been made,) should occasion no variation whatever in the form of the sulphuret. Such a circumstance would be in direct opposition to every observation that has hitherto been made on the subject.

Having been, for a long time past, impressed with an idea, that iron and copper are the only metals combined with the sulphur, in the natural composition of the gray tetraedral sulphuret

of copper, and wishing to have the truth of this opinion ascertained by chemical analysis, I desired Mr. CHENEVIX, about two years ago, to be so good as to analyse seven different specimens of the above-mentioned ore, in which I suspected the presence of antimony. I requested him, at the same time, to search with every possible care, for any metal the ore might contain besides copper and iron. Of these seven specimens, one came from Kapnick in Transylvania, one from Merkirch in Alsace, one from Andreasberg in the Hartz, one from Grossmandorf in Saxony, one from Freyberg in Saxony, one from Hesse, and one from the Alps of Dauphiny. Not one of them contained a particle of lead, or of silver; but every one of them contained antimony, although in such various proportions, as to exhibit the following differences; *viz.* $\frac{31}{100}$, $\frac{28}{100}$, $\frac{17}{100}$, $\frac{10}{100}$, and $\frac{5}{100}$. From these results, (whatever error may be supposed to have been committed in the operation,) no one, I think, can possibly consider antimony as really forming a constituent part of this sulphuret. Two of the above-mentioned specimens possessed a determinate crystalline form, namely, a tetraedron. One of them, which came from Kapnick, contained $\frac{28}{100}$ of antimony; the other, which came from Hesse, contained $\frac{10}{100}$.

In order to complete this investigation, after having given Mr. CHENEVIX some other specimens of this gray sulphuret of copper, which had no appearance of crystallization, and which he found to contain nothing but sulphur, copper, and iron, without any trace whatever of antimony, I desired him to be so good as to add to his analyses, that of some very brilliant crystals of this same sulphuret, which came from Cornwall, and which were in the form of a tetraedron, with the edges doubly bevelled, and the solid angles truncated. They also were found to contain

nothing but copper, iron, and sulphur, in the following proportions; namely, $\frac{53}{100}$ of copper, $\frac{33}{100}$ of iron, $\frac{14}{100}$ of sulphur. Perhaps we may, with some reason, consider the latter analysis, as that of this sulphuret in its purest state, and consequently as that which most decidedly declares the true nature of the tetraedral gray martial sulphuret of copper, when free from those extraneous metals that so frequently unite with it, by introducing themselves within its substance.

This gray copper ore (the *Fablerz* of the Germans) is therefore, in my opinion, shown to be nothing more than a simple combination of sulphur with copper and iron, in other words, a double sulphuret, of copper and iron; but it is very apt, during its formation, to admit other metallic substances into its composition, by the heterogeneous attraction of aggregation. When this double sulphuret is scratched with a knife, the powder obtained is sometimes of a black colour: this is always the case when the sulphuret is unmixed. At other times, the powder is of a reddish-brown colour; and it may then be presumed, that the sulphuret contains a mixture of silver and antimony, generally combined together, in the state of red silver.

I have seen many specimens of this substance, in which, by means of a lens, particles of red silver might be perceived. In some specimens, they might be seen with the naked eye. When this sulphuret is taken from a mine that contains sulphuret of lead, it very frequently contains some particles of the latter metal.

On the other hand, however, I cannot consider, as most mineralogists do at present, yellow copper ore (the *Kupferkies* of the Germans) as a mere martial pyrites, or sulphuret of iron, holding copper interp osed within its substance. This ore also

appears to me to be a double sulphuret, of copper and iron; but constituting a species distinct from the gray sulphuret of the same form. Chemistry has not yet ascertained, in a certain and satisfactory manner, the proportions of the constituent parts of this yellow double sulphuret. Its primitive form is a regular tetraedron, of which the octaedral form it sometimes exhibits, is only a modification, produced by each of its solid angles having been replaced by a plane, which is perpendicular to the axis passing through these angles. That this is really the case, is also proved, by my having seen specimens of this double sulphuret, (and chiefly among those that came from Cornwall,) which exhibited several of the well known modifications of the regular tetraedron; a circumstance that never takes place in the octaedral sulphuret of iron, even when it happens accidentally to contain a portion of copper.*

* Though I do not admit that the above-mentioned yellow copper ore, is merely a sulphuret of iron, with copper interposed within its substance, I am far from asserting, that the last mentioned sulphuret does not sometimes contain a portion of copper interposed within it; but, when that happens, the copper is generally in much smaller quantity, and its proportions are very irregular, insomuch that, in a hundred weight of sulphuret of iron containing copper, the quantity of copper varies, from a few ounces to several pounds. Besides, this sulphuret of iron, in the above-mentioned circumstances, constantly preserves the external characters that are peculiar to it; which, as we shall hereafter see, are totally different from those of the yellow double sulphuret.

But, what appears to me worthy of remark is, that when this sulphuret of iron, thus mixed with copper, assumes a determinate form, that form is always one of those belonging to the octaedral sulphuret of iron; whereas, when gold happens to be, in the above manner, interposed within the substance of this sulphuret, it is always in that kind which assumes the form of striated cubes, or in that dodecaedral modification of the above form which has pentagonal planes.

The constancy of the above facts, sufficiently shows that they are not owing to

Among the above forms, there is one in particular, which has not yet been described as belonging to this sulphuret; namely, the dodecaedron with rhombic planes, and also the passage of the tetraedron, more or less advanced, towards this dodecaedron, by each of its solid angles having been replaced by three planes, situated upon its sides. This dodecaedron, which is by no means common, even in Cornwall, (the only place where I have yet met with it,) is found there of a pretty considerable size, being sometimes an inch, or even more, in diameter. This form has never yet been observed among the sulphurets of iron.

All the other characters of the tetraedral yellow double sulphuret, are likewise different from those of the octaedral sulphuret of iron. Its yellow colour is more deep; its fracture is more brilliant; its grain is much less even, and exhibits some

chance. In like manner, as those sulphurets of iron which contain neither copper nor gold, are found in the form of octaedrons, of striated cubes, and of dodecaedrons, it is evident that the form of such sulphurets is not modified by either of the last-mentioned metals.

The above observations, in my opinion, tend very much to confirm what I have advanced with respect to the heterogeneous attraction of aggregation, which appears (in an infinite number of circumstances) to take place between the integrant molecules of one substance and others that are of a nature totally different, and gives rise to a body that is physically, but not chemically, different from what would have been produced, if these heterogeneous molecules had not been interposed within it.

Thus, even the sulphuret of iron generally contains a much larger proportion of sulphur than that which combines with the iron, during its formation. This superfluous proportion of sulphur is consequently foreign to the nature of the sulphuret, and is not necessary to its formation. It may be separated by means of distillation, without decomposing the real sulphuret; and manifests itself in a very striking manner, when, after having grossly powdered the sulphuret, a portion of the powder is thrown upon a live coal, or a red hot iron, by the inflammation which takes place, on account of this portion of the sulphur being in an uncombined state. All kinds of pyrites, when treated in this manner, emit a phosphorescent light, of a fiery red colour.

parts that are smooth, and sometimes partially conchoid; which appearances are never observed in the octaedral sulphuret of iron. The hardness of this double sulphuret is also less considerable. The octaedral sulphuret of iron scratches it with great ease; and, if we endeavour to obtain sparks from it, by means of a piece of steel, it is with great difficulty that any can be procured: it is also more brittle. If grossly powdered, and thrown upon an iron heated to redness, although it then emits a strong smell of sulphureous acid, we do not perceive that inflammation of the uncombined sulphur which, as I have already said, takes place when the octaedral sulphuret of iron is treated in the same manner; yet this last-mentioned kind of sulphuret, and that in striated cubes, are those which contain the smallest quantity of superabundant or uncombined sulphur. The powder of the yellow double sulphuret, however, when thrown upon the hot iron, emits a fiery red light, similar to that which proceeds from the sulphuret of iron; indeed its light is still more vivid. Lastly, its specific gravity is less considerable; that which I obtained from a trial of several crystals, of a perfectly determinate form, was always between 4000 and 4100; the mean being 4058. Whereas, the specific gravity of the octaedral sulphuret, taken in similar circumstances, was between 4900 and 5000; the mean being 4944.

But the yellow double sulphuret, notwithstanding it exhibits, or at least seems to exhibit, the same primitive form as the gray tetraedral sulphuret, (although it is far from admitting its various modifications,) seems to be by no means of the same nature with it, and ought, in my opinion, to be considered as forming a species which, though analogous to the other, is really different from it. The colour (a character of

infinitely more consequence in metals than in stones) of the first-mentioned sulphuret is always a bright deep yellow, whereas that of the other is a blackish gray; this circumstance alone would be sufficient to create some doubts respecting the identity of these two substances, but their other characters also present very striking differences. The gray sulphuret is harder: its powder, instead of being of a greenish-brown colour, like that of the yellow sulphuret, is black. This powder, when thrown upon an iron heated to redness, emits neither the smell of sulphureous acid, nor the beautiful phosphorescent light of the other. The specific gravity of the gray sulphuret, taken from crystals of a perfectly determinate form, was always found to be between 4460 and 4560, the mean being 4512; while that of the yellow double sulphuret is, as I have already stated, 4058. The crystal that had the highest specific gravity came from Cornwall, and belongs to the kind I have already described as containing only copper and iron combined with the sulphur; which kind, I think, ought to be considered as a standard of comparison in this species of double sulphuret. The specific gravity of the above crystal was 4558.

A question here naturally presents itself, to which, if we consider the present state of our knowledge, it appears not very easy to furnish an answer. As the true sulphuret of copper is of a blackish-gray colour, and the tetraedral gray double sulphuret (*Fahlerz*) is also of that colour, how happens it, that the yellow double sulphuret (*Kupferkies*) has always that brilliant yellow colour which characterises it, and which is at the same time the principal cause that leads many mineralogists to consider it as being nothing more than a martial pyrites mixed with copper? To answer this question, as I

have already said, appears to me by no means easy. Is it possible that the iron, which Mr. PROUST, in his Memoir upon metallic Sulphurets, has shown to be in a metallic state in the sulphuret of iron, is also in a metallic state in the yellow double sulphuret, and is, on the contrary, in the state of oxide in the gray double sulphuret? Or, can the property of emitting a beautiful phosphorescent fiery light, when thrown upon a heated iron, (which property is common both to the yellow double sulphuret, and to the martial pyrites,) have any connection with the cause upon which the colour depends? It is true that Mr. PROUST, in the memoir already referred to, says that the sulphuret of copper, when in its state of greatest purity, is of a deep blue colour, with a coppery appearance: and he states this colour to be one of the characters by which that substance may be distinguished. This observation may perhaps be true, with respect to the artificial sulphuret of copper, for I am not acquainted with that substance; but it would certainly lead astray any naturalist who should attempt to use it as a guide to distinguish the natural sulphuret of copper. The true colour of the latter substance, when in its most pure state, is a very dark gray. In that state, it assumes a peculiar crystalline form, and, when cut with a knife, exhibits a metallic lustre, as is the case with sulphuret of silver.

A portion of iron sometimes combines with the sulphuret of copper, and produces a new kind of double sulphuret, which really exhibits the colours mentioned by Mr. PROUST, and particularly the reddish colour of copper, or of nickel: it is the *Buntkupfererz* of WERNER. This new kind of double sulphuret also crystallizes, and in forms which are peculiar to it, and which are not at all analogous to those of the other double sul-

phurets; among these forms, however, the octaedron (the only form mentioned by Mr. WERNER) appears to me not to exist. To this species belongs that which, in Mr. KLAPROTH'S analysis, afforded him $\frac{50}{100}$ of copper, $\frac{35}{100}$ of iron, and $\frac{30}{100}$ of sulphur; and which Mr. WERNER makes his second sub-species of vitreous copper, under the denomination of *lamellated vitreous copper;* but it is in fact a variety of the *Buntkupfererz*, not of the vitreous copper. It is true indeed, that this latter sometimes contains a portion of iron, but always a very inconsiderable one, within its substance; and, when that happens, the iron is totally foreign to its composition. Among the analyses of the various combinations of copper with sulphur, which Mr. CHENEVIX was so good as to make at my request, (the specimens for that purpose having been furnished by me,) one was that of a very pure sulphuret of copper, which came from Cornwall, and was in crystals of a perfectly determinate form. The constituent parts of this sulphuret appeared, by the analysis, to be $\frac{81}{100}$ of copper, and $\frac{19}{100}$ of sulphur. In six others of the above-mentioned specimens, there seemed to be a mixture of iron, varying in proportion, from $\frac{3}{100}$ to $\frac{6}{100}$. Lastly, from the analysis of several specimens of the sulphuret last described, *(Buntkupfererz,)* which were of the colour of nickel, I am induced to believe that the proportions of the real constituent parts of this species, must be very nearly as follows, *viz.* from $\frac{60}{100}$ to $\frac{65}{100}$ of copper, and from $\frac{15}{100}$ to $\frac{18}{100}$ of iron, the remainder being sulphur.

From what I have here observed, which however is to be considered merely as a cursory account of the various sulphurets of copper, it may easily be inferred, that there exist many species of this substance, which have not yet been

described; also, that several of those with which we are acquainted, have not been sufficiently examined. These sulphurets are well worthy of fixing the attention of chemists. The observations to which they may give rise, are sufficiently interesting to engage their attention, and induce them to bestow particular care on their inquiries concerning them, as such inquiries may perhaps tend to increase our knowledge respecting the nature of copper, and also of iron. But I cannot too strongly recommend to those chemists who may engage in the research, to be very particular in the choice of the specimens they make use of, and also to make a great number of experiments, that they may serve as objects of comparison to each other.

Having very favourable opportunities of examining, and comparing with each other, the different kinds of copper ore that are found in the county of Cornwall, (which county furnishes a greater variety of combinations of that metal than any other part of Europe, several of which are peculiar to it,) it was impossible that my attention should not be attracted by the many interesting facts that came under my observation. Next to the combinations of this metal with the arsenical acid, the study of its sulphurets has been the principal object of my pursuits; and, if future circumstances should concur with my wishes in that respect, I hope I shall have it in my power to present to the public the result of my observations on those sulphurets.

Those observations will, I trust, furnish additional proofs of the truth of a circumstance respecting which I have long ceased to have any doubts, and which I have already, on various occasions, (particularly in my description of the arseniates of copper,) attempted to establish, namely, that there exist many minerals, which differ in species, although they have the same

substance, or collection of substances, for their basis, and are combined with the same modifying substance; and that the difference between these minerals, arises merely from the different proportions of their basis, and of their modifying substance. The more I study the works of nature, the more I become persuaded of the truth of the above remark; I am also convinced, that a want of attention to this circumstance has led mineralogists to confound one species with another.

If to the above cause of error we add that which is occasioned by neglecting, in the analysis of these substances, to distinguish those parts which (by their chemical combination and mode of attraction) determine the particular nature of the substance analysed, from those which enter into its formation in consequence of the heterogeneous attraction of aggregation, we shall readily conceive that it is the duty of chemists to correct gradually the errors they have occasioned in mineralogy. But I must repeat that, in order to correct these errors, it is necessary to make a number of comparative analyses of the substance whose nature they wish to determine. It is also necessary, that the specimens they make use of should be very perfect in their kind, taken from various districts, and, as much as possible, from various matrices. It may perhaps not be improper to add, that the interest of science requires, that the mineralogist and the chemist should mutually sanction the operations each of them, in his respective department, undertakes to perform.

Fig. 1.

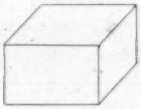


Fig. 2.

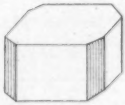
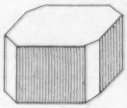


Fig. 3.



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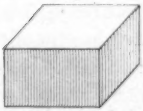

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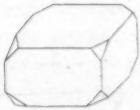

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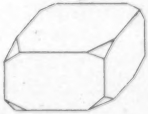

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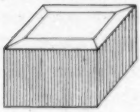

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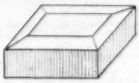

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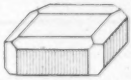

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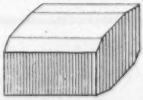

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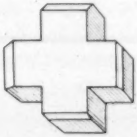

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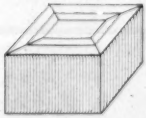


Fig. 13.

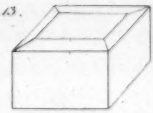
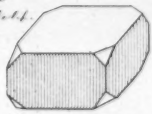


Fig. 14.



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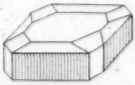


Fig. 16.

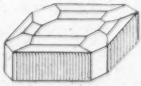
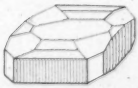


Fig. 17.



Basire fc.

V. *Analysis of a triple Sulphuret, of Lead, Antimony, and Copper, from Cornwall.* *By* Charles Hatchett, *Esq. F. R. S.*

Read January 26, 1804.

THE substance which forms the subject of this Paper, has hitherto been regarded as an ore of antimony; it is extremely rare, and has only been obtained from Huel Boys, in the parish of Endellion, a mine which, from deficiency of profit, has for some time been abandoned.

The scarcity of the ore has probably been the cause of its being unknown to foreign mineralogists; indeed few even of the British cabinets possess it; but the most perfect and beautiful specimens are (as far as I know) to be seen in the splendid collection of PHILIP RASHLEIGH, Esq. of Menabilly, in Cornwall.

To Mr. RASHLEIGH we are indebted for the first description of this ore;* but no subsequent notice had been taken of it, until the preceding Paper was written by the Count de BOURNON, whose eminent merits, as a mineralogist and crystallographer, are well known to this Society.

1.

The specific gravity of this substance is 5766, at 65° of FAHRENHEIT.

* Specimens of British Minerals, selected from the Cabinet of PHILIP RASHLEIGH, Esq. F. R. S. &c. Part I. page 34, Plate XIX.

II.

If suddenly heated on charcoal, by the blowpipe, it crackles and splits; but, when gradually exposed to the flame, it liquefies, and, upon cooling, assumes a dull metallic gray colour.

When the globule was longer exposed to heat, white fumes (which at first had a sulphureous odour) were evolved, and partly settled on the charcoal.

Ebullition prevailed during the discharge of these white fumes; and the globule gradually suffered considerable diminution, remaining at length tranquil, and of a very dark gray colour.

Upon examination, this appeared to be principally sulphuret of lead, which, like a crust, enveloped a minute globule of metallic copper, so malleable as to bear to be flattened by a hammer.

III.

Some of the ore, finely powdered, was put into a matrass, and nitric acid diluted with an equal portion of water was poured on it. Upon being digested in a low heat, a considerable part was dissolved, with much effervescence. Some sulphur, which floated, was separated; and the clear liquor, which was bluish green, was decanted from the residuum at the bottom of the vessel.

A great part of the excess of acid being expelled from the solution, it was largely diluted with distilled water, and some dissolved muriate of soda was added; but this did not produce any alteration in the transparency of the liquor. A solution of sulphate of soda was then poured in, and formed a very copious precipitate of sulphate of lead.

When this had been separated, the liquor was saturated with ammonia; by which it was changed to a deep blue colour. A

few flocculi of iron were separated; and the remainder was found to contain nothing but copper.

The sulphur which had floated, was added to the residuum which had subsided to the bottom of the matrass; and the whole was digested with muriatic acid. This solution was of a straw colour; and, when separated from the sulphur, and poured into a large quantity of water, afforded a plentiful white precipitate.

This precipitate was completely resolved into white fumes, by the blowpipe; and the muriatic solution of it, when added to water impregnated with hydro-sulphuret of ammonia, formed the orange coloured precipitate, commonly known by the appellation of golden sulphur of antimony.

IV.

Muriatic acid did not immediately act upon the pulverized ore; but a solution was speedily effected by the addition of a few drops of nitric acid: pure sulphur was separated; and the liquor, being decanted into water, yielded a copious precipitate of oxide of antimony.

The filtrated solution, by gradual evaporation, afforded crystals of muriate of lead; and the lead which afterwards remained in the liquor, was separated by a few drops of sulphuric acid.

The solution was now of a bright green colour, and, as before, was found only to contain copper, and a minute portion of iron; the former was therefore precipitated in the metallic state, by a plate of zinc.

These experiments, with others which I have not thought necessary to mention, prove, that the constituent parts of this ore are lead, antimony, copper, and a little iron, combined with

sulphur; and, when the specific gravity, the external and internal colour, fracture, grain, and other characters are considered, there can be no doubt but that at least the three first metals exist in the ore, in, or nearly in, the metallic state, combined with sulphur, so as to form a triple sulphuret; to ascertain the proportions of which, the following analysis was made.

V.

ANALYSIS.

A. 200 grains of the ore, reduced to a fine powder, were put into a glass matrass, and, two ounces of muriatic acid being added, the vessel was placed in a sand-bath. As this acid, even when heated, scarcely produced any effect, some nitric acid was gradually added, by drops, until a moderate effervescence began to appear.

The whole was then digested in a gentle heat, during one hour; and a green coloured solution was formed, whilst a quantity of sulphur floated on the surface, which was collected, and was again digested in another vessel, with half an ounce of muriatic acid.

The sulphur then appeared to be pure, and, being well washed and dried on bibulous paper, weighed 34 grains: it was afterwards burned in a porcelain cup, without leaving any other residuum than a slight dark stain.

B. The green solution, by cooling, had deposited a white saline sediment; but this disappeared upon the application of heat, and the addition of the muriatic acid in which the sulphur had been digested.

The solution was perfectly transparent, and of a yellowish

green: it was made to boil, and in this state was added to three quarts of boiling distilled water, which immediately became like milk; this was poured on a very bibulous filter, so that the liquor passed through before it had time to cool; and the white precipitate thus collected, being well edulcorated with boiling water, and dried on a sand-bath, weighed 63 grains.

C. The washings were added to the filtrated liquor; and the whole was gradually evaporated at different times, between each of which it was suffered to cool, and remain undisturbed during several hours. A quantity of crystallized muriate of lead was thus obtained, until nearly the whole of the liquor was evaporated: to this last portion a few drops of sulphuric acid were added, and the evaporation was carried on to dryness; after which, the residuum, being dissolved in boiling distilled water, left a small portion of sulphate of lead.

The crystallized muriate of lead was then dissolved in boiling water; and, being precipitated by sulphate of soda, was added to the former portion, was washed, dried on a sand-bath, and then weighed 120.20 grains.

D. The filtrated liquor was now of a pale bluish-green, which changed to deep blue, upon the addition of ammonia; some ochraceous flocculi were collected, and, when dry, were heated with wax in a porcelain crucible, by which they became completely attractable by the magnet, and weighed 2.40 grains.

E. The clear blue liquor was evaporated nearly to dryness; and, being boiled with strong lixivium of pure potash, until the whole was almost reduced to a dry mass, it was dissolved in boiling distilled water; and the black oxide of copper, being collected and washed on a filter, was completely dried, and weighed 32 grains.

200 grains of the ore, treated as here stated, afforded,

	Grains.
A. Sulphur - - -	34.
B. Oxide of antimony -	63.
C. Sulphate of lead - -	120.20
D. Iron - - -	2.40
E. Black oxide of copper -	32.

But the metals composing this triple sulphuret are evidently in the metallic state; and white oxide of antimony precipitated from muriatic acid by water, is to metallic antimony as 180 to 100; therefore, the 63 grains of the oxide must be estimated at 48.46, grains of the metal.

Again, sulphate of lead is to metallic lead as 141 to 100; therefore, 120.20 grains of the former are = 85.24 grains of the latter. And, lastly, black oxide of copper contains 20 *per cent.* of oxygen; consequently, 32 grains of the black oxide are = 25.60 grains of metallic copper.

The proportions for 200 grains of the ore, will therefore be,

Sulphur - - -	34.
Antimony - -	48.46
Lead - - -	85.24
Iron - - - -	2.40
Copper - - -	25.60
	195.70
Loss -	4.30

Or, *per cent.*

Sulphur - - -	17.
Antimony - -	24.23
Lead - - -	42.62
Iron - - -	1.20
Copper - - -	12.80
	97.85
Loss -	2.15

These proportions, I have reason to believe, are tolerably exact; for I did not observe any essential variation in the results of two other analyses, which I made of this substance, with every possible precaution.

The loss may be principally ascribed to the oxide of antimony and sulphate of lead; but especially to the former, which has a great tendency to adhere to filters and glass vessels.

In some of the preliminary experiments, I obtained a small portion of zinc; but, having received, through the kindness of Mr. R. PHILLIPS, of Lombard-street, some pure crystals of the ore, I found that the zinc had proceeded from blende, which was imperceptibly mixed in the specimens which I had first examined.

VI. *Observations on the Orifices found in certain poisonous Snakes, situated between the Nostril and the Eye. By* Patrick Russell, *M. D. F. R. S. With some Remarks on the Structure of those Orifices; and the Description of a Bag connected with the Eye, met with in the same Snakes. By* Everard Home, *Esq. F. R. S.*

Read February 2, 1804.

In the description of the *Fer-de-lance* or yellow snake of Martinico, the Count de la Cepede has remarked an orifice on each side of the head, between the nostril and the eye, which had by some naturalists been conceived to be the external organ of hearing; but, not having an opportunity himself to ascertain the fact by dissection, he recommends it as an interesting object of future investigation.*

I have, in the course of the last three years, received two colubers from Java; and, by favour of Dr. Clark, two from Martinico; all four venomous, and distinguished by lateral orifices. In the month of January, 1803, Dr. Garthshore presented me with a specimen of the yellow snake of Martinico, in excellent preservation.

Six subjects, distinguished by these lateral orifices, now in my possession, offering a fair opportunity to determine a curious circumstance in comparative anatomy, the specimens were submitted to my friend Mr. Home, of whose assistance I had more than once availed myself, in similar investigations. My request

* *Hist. Nat.* Tom. II. p. 122.

was once more attended to; and the subjoined description and remarks were received in return.

Among the specimens submitted to Mr. HOME, was one of the *Bodroo Pam*, in the description of which, lately published,* I have misrepresented the orifices now in question as the nostrils, having entirely overlooked the real nostrils.

While the anatomical disquisitions were going on, inspection was made into some of the numerous collections of serpents preserved in the museums in London. In the British Museum I was shown, exclusive of the rattle-snake and the *Fer-de-lance*, four or five colubers† with lateral orifices; in the LEVERIAN Museum, I found two or three; in the HUNTERIAN Museum, two colubers,‡ and three boæ; §¶ and in that of Mr. HEAVISIDE, one coluber. ‖

The total found in the museums above-mentioned, (exclusive of the rattle-snake,) were ten or eleven colubers, and three boæ; which, added to five colubers in my own possession, amount to eighteen or nineteen subjects furnished with lateral orifices.

It appears, on the whole, that the lateral orifices have hitherto been found only in venomous serpents.

That (exclusive of the rattle-snake) they have been found in fifteen or sixteen species of colubers, and in three of the genus boa.

That they have not as yet been discovered in any of the genus anguis.

Mr. HOME'S investigations have clearly established, that these

* Account of Indian Serpents collected on the Coast of Coromandel, No. IX. Lond. 1796.

† All, I believe, non-descripts.

‡ No. 977, 1058.

§ No. 893, 1016, 1046.

‖ No. 64.

lateral orifices in serpents, and the bags to which they lead, have no communication with the organ of hearing. Another fact ascertained by him is, that serpents distinguished by lateral orifices, have a cavity situated between the bag and the eye, which, so far as I know, has not been observed before.

Mr. HOME'S *Description, and Remarks.*

The orifices situated between the eye and the nostril, in the rattle-snake, and in some species of coluber, do not lead to the nostril or to the ear, but to a distinct bag, of a rounded form; there is a hollow of the same shape surrounded by bone, and adapted to receive it. Dr. TYSON'S description of the rattle-snake is tolerably accurate: he says, "between the nostrils and "the eyes, but somewhat lower, were two orifices, which I took "for the ears; but after, I found they only led into a bone, that "had a pretty large cavity, but no perforation." *

The cavity which Dr. TYSON describes to be in the bone, is a cup, formed by the bones of the skull and those of the upper jaw; it is in shape not unlike the orbit, and is formed in a similar manner.

These bags bear a relative proportion to the size of the snake; they are lined, as also the eyelids, with a cuticle, which forms the transparent cornea, making a part of the outer cuticle, and is shed with it; and, when examined after the snake has cast it off, their shape is more perfectly seen than under any other circumstances.

In the annexed figures, one of these bags is represented in different views; all of them of the natural size, both in the *Fer-de-lance* or yellow snake of Martinico, and in the detached

* Phil. Trans. Vol. XIII. p. 26.

cuticle of the rattle-snake. The appearance in the *Bodroo Pam* is exactly the same; but, as the bag in that snake is of a smaller size, it was considered unnecessary to give a representation of it.

In the deer and antelope there are bags, in the same relative situation respecting the eye and the nose, resting upon the skull; there is also a cavity in the bone, adapted to receive them. The bags vary in size in the different species of these genera. The French naturalists have given the name of *larmiers* to these bags, conceiving them to be receptacles for the tears, of which the thinner parts evaporating, a substance remains called *larmes de cerf*.

I requested my friend Mr. ANDRE to examine these bags in the common buck, and to observe their relative position to the puncta lachrymalia; his situation in the Earl of EGREMONT's family, at Petworth, affording him every opportunity for doing it. He informs me, that the bags are lined with a cuticle, similar to that of the meatus auditorius externus in the human ear; their internal surface is smooth, free from hair, and without any appearance of glandular structure. From the inner angle of the eye to this bag, there is a kind of gutter in the skin, of a darker colour than the rest of the skin in light coloured animals, and the hairs are shorter than on the rest of the body: the substance contained in the bags resembled that found in the ears.

The lachrymal gland in the deer, he says, is very large, and the puncta so much so, as to admit the rounded end of a common probe. There is no lachrymal sac; the tubes from the puncta unite, and pass through a small opening in the bone, to the nose.

The following account of these bags, in the antelope of Sumatra, was transmitted to me in the year 1792, by Mr. BELL.

"The external orifice is of the size of a crow-quill; it leads into "a bag not larger than a small marble, which is lined with a "cuticle, with hair. From this bag there is a secretion of a "limpid fluid, which keeps oozing down the nose." This gentleman, unfortunately for natural history, died at Sumatra, soon after the date of his letter.

In the HUNTERIAN Museum, intrusted by government to the care of the College of Surgeons, there are several specimens of these bags, from the Egyptian antelope with annulated horns, and also from some other species: these are preserved so as to show the internal cavity of the bag, and the structure of the gland immediately behind it. In these specimens, the glandular part is $\frac{1}{4}$ of an inch in thickness; from the centre of this gland, an excretory duct opens into the bag, immediately opposite to the external orifice. The bag itself is lined with a cuticle, and thinly set with strong hairs.

The facts now produced are sufficient to prove that these bags have a secretion of their own, the quantity of which varies, according to the climate and other circumstances; and there is no reason for thinking that the tears ever pass into them, the passage into the nose being unusually free, and the orifices in the bags, in many species, unfavourably situated for the reception of the tears.

We are at present unacquainted with the use to which the fluid secreted in these bags is applied.

As amphibious animals, in general, have no glands to supply the skin with moisture from within, but receive it by coming in contact with moist substances, it is possible the bags, in the snake, may be supplied in that manner, and the more so, as the cuticular lining appears perfect.

Another peculiarity is remarkable in snakes furnished with the bags described above, namely, an oval cavity, situated between the bag and the eye, the opening into which is within the inner angle of the eyelid, and directed towards the cornea. In this opening there are two rows of projections, which appear to form an orifice, capable of dilatation and contraction. From the situation of these oval cavities, they must be considered as reservoirs for a fluid, which is occasionally to be spread over the cornea; and they may be filled by the falling of the dew, or the moisture shaken off from the grass through which the snake passes.

This apparatus in the snake, has that position which is best adapted to pour out the fluid upon the cornea, when the head of the snake is erect.

Dr. TYSON had superficially observed the apparatus which has been described, and considered it as a membrana nictitans. He says, "inwards it seemed to have a membrana nictitans, "which removes any dust that might adhere to the eye."*

As snakes in general have no apparatus to wash the cornea, these particular species must have some peculiarities in their mode of life, with which we are not at present acquainted.

* Phil. Trans. Vol. XIII. p. 27.

EXPLANATION OF THE FIGURES. SEE PLATE III.

Fig. 1. Represents a side view of the head of the *Fer-de-lance* or yellow snake of Martinico, to show the external appearance of the orifice, with its relative situation to the nostril and the eye. The parts are delineated of their natural size.

Fig. 2. A side view of the head of the same snake, in which the bag is laid open. At the aperture of the cavity, which opens towards the cornea, there is a double row of small projecting points.

Fig. 3. The cuticle of the rattle-snake, after it had been cast off from one side of the head, represented of its natural dimensions. In this view, the internal surface only of the cuticle is seen. There is an aperture, of an irregularly oval form, which is the opening of the nostril: a little farther on is the lining of the rounded bag, in a distended state; nearer the eye is the cavity communicating with the space before the cornea, it is of an oval form, and has a narrow neck; beyond this neck is the transparent cornea, which in the snake is cuticular, and is shed with the external covering of the other parts. Through the transparent cornea, a bristle is seen passing before its external surface into the cavity.

This figure is taken from a preparation in the HUNTERIAN Museum.

Fig. 1.

Fig. 2.

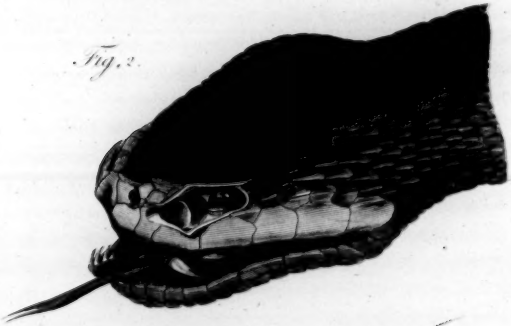


Fig. 3.

W. Clift del.

Basire sc.

VII. *An Enquiry concerning the Nature of Heat, and the Mode of its Communication.* *By* Benjamin *Count of* Rumford, *V.P.R.S. Foreign Associate of the National Institute of France, &c.*

Read February 9, 1804.

HEAT is employed in such a vast variety of different processes, in the affairs of life, that every new discovery relative to it must necessarily be of real importance to mankind; for, by obtaining a more intimate knowledge of its nature and mode of action, we shall no doubt be enabled not only to excite it with greater economy, but also to confine it with greater facility, and direct its operations with more precision and effect.

Having many years ago found reason to conclude, that a careful observation of the phenomena which attend the heating and cooling of bodies, or the communication of heat from one body to another, would afford the best chance of acquiring a farther insight into the nature of heat, my view, in all my researches on this subject, has been principally directed to that point; and the experiments of which I am now to give an account, may be considered as a continuation of those I have already, at different times, had the honour of laying before the Royal Society, and of presenting to the public in my Essays.

In order that the attention of the Society may not be interrupted unnecessarily, by descriptions of instruments, in the midst of the accounts of interesting experiments, I shall begin by describing the apparatus which was provided for these researches;

and, as a perfect knowledge of the instruments made use of, is indispensably necessary, in order to form distinct ideas of the experiments, I shall take the liberty to be very particular in these descriptions.

The thermometers, four in number, which were used in these experiments, were constructed under my own eye, and with the greatest possible care; and, after every trial I have been able to make with them, in order to ascertain their accuracy, they appear to be very perfect.

They are mercurial thermometers, graduated according to FAHRENHEIT: their bulbs are cylindrical, 4 inches long, and $\frac{4}{10}$ of an inch in diameter; and their tubes are from 15 to 16 inches long. The mercury with which they are filled is quite pure; and they are freed from air. Their scales were divided with the greatest care; and, by means of a nonius, they show eighth parts of a degree very distinctly: they are graduated from about 10 degrees below the freezing point, to 5 or 6 degrees above the point of boiling water. Their bulbs are quite naked; their scales ending about 1 inch above the junction of the bulb with its tube. The freezing point is situated about 5 inches above the upper end of the bulb. The reason for placing it so high, will be evident, from the details of the experiments in which these instruments were used.

The instrument I contrived for ascertaining the warmth of clothing, is extremely simple: it is merely a hollow cylindrical vessel, made of thin sheet brass. It is closed at both ends; and has a narrow cylindrical neck, by which it is occasionally filled with hot water.

This vessel, being covered with a garment made to fit it,

composed of any kind of cloth, or stuff, or other warm covering, is supported, in a vertical position, on a wooden stand, which is placed on a table, in a large quiet room; and, one of the thermometers above described being placed in the axis of the vessel, the time employed in cooling the water, through the clothing with which the instrument is covered, is observed and noted down.

Now, as the time of cooling through any given interval of the scale of the thermometer, (or from any given degree above the temperature of the air of the room, to any other given lower degree, but still above the temperature of the air of the room,) will be longer, or shorter, as the covering of the instrument is more or less adapted for confining heat, it is evident, that the relative warmth of clothing of different kinds, may be very accurately determined by experiments of this sort.

I provided four instruments of this kind, all very nearly of the same dimensions. Their cylindrical bodies are each 4 inches in diameter, and 4 inches long; and their cylindrical necks are about $\frac{1}{10}$ of an inch in diameter, and 4 inches in length. This neck is placed in the centre of the circular flat top, or upper end, of the vertical cylindrical body; and, opposite to it, in the centre of the flat bottom of the body, there is a hollow cylinder, $\frac{1}{10}$ of an inch in diameter, and 3 inches long, projecting downwards, into which a vertical cylinder of wood is fitted, on the top of which the instrument is supported, in such a manner that the air has free access to every part of it. This cylinder of wood constitutes a part of the wooden stand above-mentioned.

As the thermometer is placed in the axis of the cylindrical vessel, and as its bulb is just as long as the body of this vessel, it is evident that it must ever indicate the *mean temperature* of

the water in the vessel, however different the temperature of that water may be at different depths.

The thermometer is firmly supported in its place, by causing a part of the lower end of its scale to enter the neck of the cylindrical vessel, and to fit it with some degree of accuracy, but not so nicely as to be in danger of sticking fast in it.

The lower end of the bulb of the thermometer does not absolutely touch the bottom of the vessel, but it is very near touching it.

Figure 1 (Plate IV.) will give a clear idea of this instrument, placed on its wooden stand, which is so contrived, that the instrument may be placed higher, or lower, at pleasure.

The foregoing description of this instrument is so particular, that the figure will be easily understood, without any farther illustration. The cylindrical vessel is represented placed on the stand, with its thermometer in its place.

As, in some of the first experiments I made with this instrument, I found it difficult to apply the coverings which I used, to the ends of the body of the instrument, I endeavoured, by covering up those ends with a permanent and very warm covering, to oblige most of the heat to pass off through the vertical sides of the instrument; to which it was easy to fit almost any kind of covering, and more especially coverings of various thicknesses of confined air, the relative warmth of which I was very desirous of ascertaining.

The means I employed for covering up the ends of the instrument were as follows. Having provided two thin cylindrical wooden boxes, (like common pill-boxes, but much larger,) something less in diameter than the body of the instrument, and $2\frac{1}{2}$ inches deep, I dried them as much as possible; and, after

having varnished them, within and without, with spirit varnish, I covered them, within and without, with fine wove writing-paper, and then gave the paper three coats of the same varnish. I then perforated the bottoms of these boxes with round holes, just large enough to admit the neck of the instrument, and the cylindrical projection at its bottom; and then inverted them over the two ends of the instrument, filling the boxes at the same time with *eider-down*.

These boxes were fixed and confined in their places, by means easy to be imagined; and, in order to confine the heat still more effectually, each of the boxes was covered on the outside with a cap of fur, as often as the instrument was used; as was also that part of the neck of the instrument which projected above the box.

Two of the instruments, which I shall distinguish by the numbers 1 and 2, were covered up at their ends in this manner: the other two instruments, No. 3 and No. 4, were left in the state represented by the Figure 1; that is to say, the ends of their cylindrical bodies were not covered with permanent coverings.

In each experiment, two similar instruments (No. 1 and No. 2, for instance, or No. 3 and No. 4) were used, the one *naked*, and the other *covered;* and, as the naked instrument always served as a standard, with which the results of the experiments made with the other were compared, it is evident, that this arrangement rendered the general results of the experiments much more satisfactory and conclusive than they could possibly have been, had the experiments made on different days, and with various kinds of covering, been made singly, or unaccompanied by a fixed and invariable standard.

The experiments were made, and registered, in the following

manner: the two instruments used in the experiment, placed on their wooden stands, being set down on the floor, were filled to within about $1\frac{1}{2}$ inch of the tops of their cylindrical necks with boiling hot water; and, a thermometer being put into each of them, they were placed, at the distance of 3 feet from each other, on a large table, in a corner of a large quiet room,* where they were suffered to cool, undisturbed. Near them, on the same table, and at the same height above the table, there was placed another thermometer, (suspended in the air, to the arm of a stand,) by which the temperature of the air of the room was ascertained from time to time.

No person was permitted to pass through the room, while an experiment was going on; and, in order to prevent, as far as it was possible, all those currents of air in the room which were occasioned by partial heat, produced by the light which came in at the windows, the window-shutters were kept constantly shut; one of them only being opened for a moment, now and then, just to observe the thermometers, and note down the progress of the experiment.

The results of each experiment were entered on a separate sheet of paper; which paper was previously prepared for that use, by being divided into separate vertical columns, by lines drawn with a pen, and ruled in parallel horizontal lines with a lead pencil.

The following is an exact copy of one of these register-sheets; and contains the results of an actual and very interesting experiment, which lasted 26 hours.

* This room, which is adjoining to my laboratory, in my house at Munich, is 19 feet wide, 24 feet long, and 13 feet high.

"Experiments on Heat, made at Munich, 11th March, 1803.

"The large cylindrical Vessels, No. 1 and No. 2, (made of thin "sheet brass,) were filled with hot Water, and exposed to "cool in the Air of a large quiet Room. The Ends of both "these Instruments were well covered with warm Clothing, "Furs, &c. The vertical polished Sides of No. 1 were *naked*. "The Sides of No. 2 were *covered* with one Thickness of fine "white *Irish Linen*, which had been worn, strained over the "metallic Surface."

Time.		Temperature		Temperature of the air.	Time.		Temperature		Temperature of the air.
h.	min.	of No. 1, *naked.*	of No. 2, *covered.*		h.	min.	of No. 1, *naked.*	of No. 2, *covered.*	
10	10	126½°	126°	43¼°	4	—	61¼°	53½°	49½°
—	30	109½	106½	43½	—	80	59½	52	—
—	45	105	100⅛	43¾	5	30	57	49¾	42½
11	—	101¼	94¾	44	6	—	55½	49⅛	—
—	2½	—	94	—	—	30	54¼	48¼	—
—	15	97½	90¼	—	7	—	53½	47½	42
—	30	94	86¼	—	8	—	51½	46½	—
—	39	—	84	—	9	—	50	45¾	—
—	45	91¼	82½	—	10	—	49	45	—
12	—	88½	79⅜	—	8	12th Mar.	48	42	40
—	15	85½	76	—	The instruments were now removed into a warm room.				
—	25	84	—	—	8	2	43	42	62
—	30	—	74½	—	—	32	44¾	44¾	62½
—	45	80	70	—	—	47	46	46½	63
1	—	78	68⅛	—	9	24	48	49½	—
—	30	74¼	64½	—	10	—	50	52	—
2	—	71⅛	61½	43¾	—	41	51½	53⅞	—
—	30	68⅛	58¾	43½	12	—	54	56½	—
3	—	65¾	56¼	—	12	26	54½	57	—
—	30	63½	54¾	—	An end was now put to the experiment.				

Though it was easy to discover, by a single glance at the register, whether a covering which was put over one of the instruments prolonged the time of its cooling or not; yet, in order to compare the results of different experiments, and particularly of such as were made on different days, so as to determine with precision *how much* warmer one kind of covering was than another, it was necessary to fix on some particular interval in the scale of the thermometer, or number of degrees, commencing at some certain invariable number of degrees above the temperature of the air by which the instrument was surrounded, in order that the warmth of the covering, or its power of confining heat, might with certainty be estimated by the time employed in cooling through that interval.

By the results of a great number of experiments I found, that the same instrument cooled through any given (small) number of degrees, (10 degrees, for instance,) in very nearly the same time, whatever was the temperature of the air of the room; provided always, that the point from which these 10 degrees commenced, was at the same given number of degrees above the temperature of the air at the time being.

The interval I chose for comparing the results of my experiments, is that which commences with the *fiftieth*, and ends with the *fortieth* degree of FAHRENHEIT's thermometer, *above the temperature of the air in which the instrument is exposed to cool.* When, for instance, the air was at 58°, the interval commenced at the 108th degree, and ended at the 98th. When the air was at $64\frac{1}{2}°$, it commenced at $114\frac{1}{2}°$, and ended at $104\frac{1}{2}°$.

That the same instrument, exposed to cool in the air, does in fact cool the same number of degrees in the same time, very nearly, when the given interval of the scale of the thermometer

is reckoned from the same height, or given number of degrees above the temperature of the air at the time when the experiment is made, will appear from the following results of 11 different experiments, made on different days, and when the air in which the instrument was exposed to cool was at different degrees of temperature.

The large cylindrical vessel, No. 1, having its two ends well covered up with eider-down, furs, &c. its vertical sides being exposed *naked* to the air, in a large quiet room, was found to cool 10 degrees, *viz.* from the 50th to the 40th degree above the temperature of the air in which it was exposed, as follows.

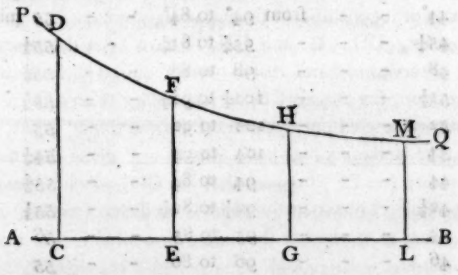
Temperature of the air.	Degrees cooled.	Time employed in cooling.
44°	from 94° to 84°	55 minutes.
45¼	95¼ to 85¼	55½
48	98 to 88	55¼
51½	101½ to 91½	55½
52	102 to 92	55
54	104 to 94	54¼
44	94 to 84	55⅝
42½	92½ to 82½	55⅓
45	95 to 85	56
46	96 to 86	55
44	94 to 84	55⅓

The fact which these experiments are here brought to prove, has likewise been confirmed by other experiments, made with other instruments, and at times when the temperature of the air has been as high as 64°; but I will not take up the time of the Society, by giving a particular account of them in this place.

As it sometimes happened, though very seldom, in the course

of an experiment, (which commonly lasted several hours,) that I was called away, and was not present to observe the thermometer, at the moment of the passage of the mercury through one or both of those points of its scale which formed the limits of the given interval, chosen as the standard for a comparison of the results of the experiments with each other, it became a matter of considerable importance, to find means for supplying these accidental defects, and ascertaining the points in question by interpolation.

In order to facilitate the means of doing this, I endeavoured to investigate the law of the cooling of hot bodies in a cold fluid medium; and I found reason to conclude,



That if, on the right line AB, a perpendicular CD be taken, equal to the difference of the temperatures of the hot body and of the colder medium, expressed in degrees of the thermometer; and, after a certain given time, represented by CE, taken on the line AB, at the point E, another perpendicular EF be erected, and EF be taken equal to the difference of the temperatures after the time represented by CE has elapsed; and if the

perpendiculars GH and LM be drawn, representing the difference of the temperatures after the times EG and GL have elapsed, a curved line PQ, drawn through the points D, F, H, M, will be the logarithmic curve; or, if it vary from that curve, its variation, within the limits answering to a change of temperature amounting to a few degrees, (especially if they be taken when the temperature of the hot body is about 40 or 50 degrees above that of the medium,) will be so very small, that no sensible error will result from a supposition that it is the logarithmic curve, in supplying, by computation, any intermediate observations, which happen to have been neglected in making an experiment.

These computations are very easily made, with the assistance of a table of logarithms, in the following manner.

Supposing CD, CG, and GH, to have been determined by actual observation; and that it were required to ascertain, by computation, the abscis CE, corresponding to any given intermediate ordinate EF, or, (which is the same thing,) to determine at *what time* the cooling body was at any given intermediate temperature (= EF) between that (= CD) which it was found by observation to have at the point C, and that (= GH) which it was found to have after the time represented by the line CG had elapsed;

It is log. CD — log. GH, is to CG as 1 to m; (= modulus = the subtangent of the curve at the point D.)* And CE = m × log. CD — log. EF.

* The subtangent shows in what time the instrument would cool down to the temperature of the air in which it is placed, were its velocity of cooling at the point D to be continued *uniformly* from that point; and, as the subtangent of the logarithmic curve is *constant*, if PQ were the logarithmic curve, it would follow, that the velocity

If, for instance, in the experiment of the 11th March, (the details of which have just been given,) the time when the instrument No. 2, in cooling, passed the important point of 94°, had not been observed, this neglect might have been supplied, by computation, in the following manner.

It is CD = $94\frac{1}{4}°$, the nearest *observed* temperature higher than EF (= 94°) and GH = $90\frac{1}{4}°$, the nearest observed temperature below that of 94°; and CG = 15 minutes, or 900 seconds, = the time elapsed between the two observations.

It is log. $94\frac{1}{4}$ = 1.9765792
And log. $90\frac{1}{4}$ = 1.9554472
Log. CD — log. GH = 0.0211320

And 0.0211320 is to 900 (= CG) as 1 to 42590 = *m*.

And again, log. $94\frac{1}{4}$ = 1.9765792
Log. 94 = 1.9751279
Log. CD — log. EF = 0.0034513

42590 × 0.0034513 (= *m* × log. CD — log. EF) = 147 seconds, = 2 minutes and 27 seconds; which differs very little from $2\frac{1}{2}$ minutes, the observed time.

If, from the temperature observed at 11^h 30 min. = $86\frac{1}{4}°$, and the temperature observed at 11^h 45 min. = $82\frac{1}{2}°$, and the time which elapsed between these two observations, (= 15 minutes) we were to determine, by computation, the time when the instrument was at the temperature of 84°, (the lower point of the standard interval of 10 degrees answering to the temperature of

with which a hot body cools in a fluid medium, is every where such, that were *that velocity* to be continued uniformly, the body would be cooled down to the temperature of the medium, *in the same time*, whatever might be the excess of the temperature of the hot body above that of the medium, at the moment when its velocity of cooling became uniform.

the air, $=44°$, in which the instrument was cooled,) it will turn out, 8 minutes and 55 seconds after 11^h 30 min. The observed time was 11^h 39 minutes; which differs from the computed time no more than 5 seconds.

If it were strictly true, as a very great philosopher and mathematician has advanced, that the velocity with which a hot body exposed to cool in a cold fluid medium parts with its heat, is as the difference of the temperatures of the body and of the medium, it is most certain, that the curve PQ could be no other than the logarithmic curve. Perhaps it may be so in fact, and that the variations from it which my experiments indicated, were owing solely to the imperfection of the divisions of our thermometers. If it be so, it is not impossible to divide the scale of a thermometer in such a manner as to indicate with certainty *equal increments of heat*, as thermometers ought to do; but this is not the proper place to enlarge on this subject. I may perhaps return to it hereafter.

Passing over in silence, a number of experiments I made in order to get thoroughly acquainted with my new instruments, and to assure myself that the results of similar experiments made with them were uniform, and might be depended on, I shall now proceed to give an account of several experiments made with pointed views, the results of some of which were very interesting.

Experiment No. 1. The large cylindrical vessel No. 1, with its ends covered with warm clothing, in the manner before described, and its vertical sides (which were polished, and very clean and bright) exposed naked to the air, was filled with water nearly boiling hot, and placed on its wooden stand, on a table, in a

large quiet room, to cool; the air of the room being at the temperature of 45° FAHRENHEIT.

Another cylindrical vessel, No. 2, in all respects like No. 1, and with its ends covered in the same manner, but with its vertical sides covered with a single covering of fine Irish linen, (such as is sold in London for about 4*s. per* yard,) closely applied to the body of the instrument, was filled with hot water at the same time, and placed on the same table to cool.

This experiment lasted many hours; and, in that period, the temperature of the water, in each of the instruments, was carefully observed, and noted down, a great number of times.

The result of this experiment (the details of which have already been given) was very remarkable.

While the instrument No. 1, whose sides were *naked*, employed 55 minutes in cooling from the point of 94° to that of 84°, the instrument No. 2, whose sides were *covered with linen*, cooled through the same interval in 86½ minutes.

Hence it appears, that clothing may, in some cases, expedite the passage of heat out of a hot body, instead of confining it in it.

Desirous of seeing whether the same covering would, or would not, expedite the passage of heat *into* the instrument; after having suffered both instruments to cool down to the temperature of about 42°, I removed them into a warm room, in which the air was at the temperature of 62°; and I found that the instrument No. 2, which was clothed, acquired heat considerably faster than the other, No. 1, which was naked.*

* The details of this experiment (which was made on the 11th of March, 1803) may be seen in page 83.

The discovery of these extraordinary facts surprised me, and excited all my curiosity; and I immediately set about investigating their cause.

As it is well known that air adheres with considerable obstinacy to the surfaces of some solid bodies, I conceived it to be possible, that the particles of air in immediate contact with the surface of the cylindrical vessel No. 1, might in fact be so attached to the metal as to adhere to it with some considerable force; and, if that were the case, as confined air is known to constitute a very warm covering, it appeared to me to be possible, that the cooling of the vessel No. 1, might have been retarded by such an invisible covering of confined air; which covering, in the experiment with the vessel No. 2, had been displaced, and in a great measure driven away, by the colder covering, of linen, by which the body of the instrument was closely embraced.

I conceived that the linen must have accelerated the cooling of the instrument, either by facilitating the approach of a succession of fresh particles of cold air, or by increasing the effects of *radiation;* and, with a view to elucidate that important point, the following experiments were made.

Exper. No. 2. Removing the linen with which the instrument No. 2 was clothed, I now covered the sides of that instrument with a thin transparent coating of glue; and, when it was quite dry and hard, I again filled the two instruments (No. 1 and No. 2) with hot water, and observed the times of their cooling as before.

Result, or time of cooling 10 degrees, reckoned from the 50th to the 40th degree above the temperature of the air in which the instruments were exposed to cool.

Instrument No. 1, sides *naked* - - - 55 min.

Instrument No. 2, sides *covered with one coating of glue* 43¼ min.

When we consider this experiment with attention, we shall find reason to conclude, that if it were by facilitating the approach and temporary contact of a succession of fresh particles of the cold air of the room to the surface of the glue, (which was now in fact become the surface of the hot body,) that the cooling of the instrument was accelerated, the metal being as completely covered, and the air, supposed to be attached and fixed to its surface, as completely excluded by one coating of the glue as it could be by two, or more, two coatings could not possibly accelerate the cooling of the instrument more than one; but if, on the other hand, the cooling of the instrument in this experiment was accelerated, not by facilitating and accelerating the motions of the circumambient cold air, but by facilitating and increasing those *radiations* which are known to proceed from hot bodies, I conceived that two coatings of the glue might possibly accelerate the cooling of the vessel more than one. In order to put this conjecture to the test, I made the following decisive experiment.

Exper. No. 3. I now gave the instrument No. 2 a second coating of glue; and, when it was thoroughly dry, I repeated the experiment last mentioned, with the above variation; when I found the results to be as follows.

	Time of cooling the 10 degrees in question.
The instrument No. 1, *naked* metal - -	$55\frac{1}{2}$ min.
No. 2, *covered* with two coatings of glue	$37\frac{1}{2}$ min.

Finding that two transparent coatings of glue facilitated the cooling of this instrument even more than one coating, I washed off all the glue with warm water; then, making the instrument as clean and bright as possible, I covered its sides with

a coating of very fine, transparent, and colourless spirit varnish; and, after this coating of varnish had become quite dry and hard, I repeated the experiment above-mentioned; and, finding that this covering, like that of glue, expedited the cooling of the instrument, I first added a *second* coating of the varnish, and repeated the experiment again, and then added two coatings more, making *four* in all. Finding that the cooling of the instrument was more and more rapid, as the thickness of the varnish was increased, I now added four coatings more, making *eight* coatings in the whole, giving time for each new coating to dry thoroughly, before the next was applied; but I found, on repeating the experiment with this thick covering of varnish, that I had passed the limit of thickness which produced the greatest effect.

In order that the results of these experiments, with coatings of different thicknesses of spirit varnish, may be seen at one view, I shall here place them all together; and I shall place by the side of each, the result of the standard experiment, which was made at the same time, with the instrument No. 1, the sides of which were *naked*.

		Time employed in cooling through the given interval of 10 degrees. Instrument No. 1, *varnished*.	Instrument No. 2, *naked*.
Exper. No. 4.	1 coating of varnish -	42 min. -	$55\frac{1}{2}$ min.
Exper. No. 5.	2 coatings - - -	$35\frac{3}{4}$ - -	$55\frac{1}{4}$
Exper. No. 6.	4 coatings - - -	$30\frac{1}{4}$ - -	$55\frac{1}{2}$
Exper. No. 7.	8 coatings - - -	$34\frac{1}{2}$ - -	55

Exper. No. 8. Desirous of finding out what effect *colour* would produce, I now painted the sides of the instrument No. 2 *black*, with lamp-black mixed up with size, (this paint being laid

upon the eighth coating of the varnish,) and, repeating the experiment, its results were as follows.

	Time employed in cooling through the given interval.
The instrument No. 1, *naked* - - -	$55\frac{1}{4}$ min.
Instrument No. 2, covered with 8 coatings of varnish, and painted *black*	34 min.

Exper. No. 9. Finding that the painting of this thick coating of varnish *black*, rendered the covering still colder, or accelerated the cooling of the instrument, I now washed off the black paint, with warm water; then, washing off all the varnish with hot spirit of wine, I painted the metallic sides of the instrument of a black colour, with lamp-black and size; and, when the paint was quite dry, I repeated the experiment so often mentioned; when the results were as follows.

	Time employed in cooling through the given interval.
The instrument No. 1, sides *naked* - -	$55\frac{1}{6}$ min.
No. 2, *painted black* - -	35 min.

Exper. No. 10. In order to find out whether the *black* colour had any particular efficacy in expediting the cooling of the instrument, or whether another colouring substance would not produce the same effect, when mixed up with the same size, I now washed off the black paint, and painted the sides of the instrument *white*, with whiting mixed up with size; and, on repeating the experiment, the results were as follows.

	Time of cooling through the given interval.
The instrument No. 1, *naked* - - -	$55\frac{1}{3}$ min.
No. 2, *painted white* - -	36 min.

As, in both the two last experiments, it was found necessary

to paint the body of the instrument three or four times over, in order to cover the polished metal so completely as to prevent its shining through the paint; this of course occasioned the surface of the metal to be covered with a thick coating of size, which, no doubt, affected very sensibly the results of the experiment, and rendered it impossible to determine, in a satisfactory manner, what the effects really were, which were produced by the *different colours* used in the two experiments.

Exper. No. 11. With a view to throw some more light on this interesting subject, having washed off the paint from the instrument No. 2, I now rendered its sides of a perfectly deep black colour, by holding it over the flame of a wax candle; and, repeating the usual experiment, the results were as follows.

	Time of cooling through the standard interval.
The instrument No. 1, *naked* - - -	$55\frac{7}{8}$ min.
No. 2, *blackened* - - -	$36\frac{1}{8}$ min.

In order to ascertain the quantity of matter which composed this black covering, I weighed a small piece of clean and very fine linen; and, having wiped off with it all the black matter from the body of the instrument No. 2, in such a manner that the whole of it remained attached to the linen, I weighed it again, and by that means discovered that the whole of this black substance, which had so completely covered the sides of the instrument (a surface of polished brass = 50 superficial inches) that the metal did not shine through it in any part, weighed no more than $\frac{1}{18}$ of a grain Troy.

How this very thin covering, which, if the specific gravity of the black matter were only equal to that of water, would amount to no more than $\frac{1}{4\,5\,0\,9}$ of an inch in thickness, could expedite

the cooling of the instrument, in the manner it was found to do, is what still remains to be shown: but, before I proceed any farther in these abstruse enquiries, I shall make a few observations relative to the results of the foregoing experiments.

Although we may with safety presume, that the velocities with which the heat escaped *through the sides of the instruments,** were nearly as the times inversely taken up in cooling through the given interval of 10 degrees; yet, as some heat must have made its way, in the course of the experiment, *through the ends of the instrument*, notwithstanding all the care that was taken to prevent it, by covering them up with warm clothing, it is necessary, in order to be able to compare the results of the preceding experiments in a satisfactory manner, to find out how much of the heat made its escape through the covered ends of the instruments, during the time the instruments were cooling through the interval in question.

In order to determine that point, I now removed the covering from the ends of the instrument No. 1; and, when it was quite naked, I found, on making the experiment, that it cooled through the given interval in $45\frac{1}{2}$ minutes.

When its two ends and its cylindrical neck were covered up

* I have found myself obliged in this, as in many other places, to make use of language which is far from being as correct as I could wish. I do not believe that heat ever *makes its escape* in the manner here indicated; but I could not venture to use uncommon expressions, in pointing out the phenomena in question, however well adapted such expressions might be to describe the events which really take place. If it should be found that *caloric*, like *phlogiston*, is merely a creature of the imagination, and has no real existence, (which has ever appeared to me to be extremely probable,) in that case, it must be incorrect to speak of heat as *making its escape* out of one body, and *passing* into another: but how often are we obliged to use incorrect and figurative language, in speaking of natural phenomena!

with warm clothing, I found, by taking the mean of the results of several experiments, that it required $55\frac{1}{2}$ minutes to cool through the same interval.

On measuring the instrument with care, I found its dimensions to be as follows.

	Inches.
Diameter of the body of the instrument -	= 4.03
Length of the body - - - -	= 3.96
Diameter of the neck of the instrument -	= 0.8
Length of the neck - - - -	= 4.

The superficies of the different parts of the instrument are therefore as follows.

Superficies of the vertical sides of the body ($= 4.03 \times 3.14159 \times 3.96$) = 50.136 inches.

Superficies of the flat circular bottom of the instrument, $\left(= 4.03 \times 3.14159 \times \frac{4.03}{4}\right)$ = 12.755 inches; deducting nothing for that part which is covered by the end of the tube, which serves as a support for the instrument.

Superficies of the flat circular top of the instrument, (after deducting 0.502 of a superficial inch, for the circular hole in its centre, made to receive the lower end of the cylindrical neck,) = 12.253 inches.

Superficies of the cylindrical neck of the instrument ($= 0.8 \times 3.14159 \times 4$) = 10.051 inches.

Supposing now, that the heat passes with equal velocity through the surface of all the different parts of the instrument, when the instrument is naked, we can determine the quantity of heat which escaped through the ends and neck of the instrument, in the experiments in which those parts of the instrument were covered with warm clothing.

The whole of the metallic surface exposed to the air, in the experiments made with the instrument when it was quite naked, amounted to 85.195 superficial inches; namely;

Surface of its vertical sides - -	= 50.136 inches.
of its lower end - -	= 12.755
of its upper end - -	= 12.253
of its neck - - -	= 10.051
Total surface =	85.195 inches.

When the instrument was exposed quite naked to the air, it was found to cool through the standard interval of 10 degrees, in $45\frac{1}{2}$ minutes.

Assuming now any given number, as the measure of the whole quantity of heat given off by the instrument during the period above-mentioned, we can ascertain what part or proportion of that quantity passed off through the sides of the instrument; and what part of it must have made its escape through its ends, and through the sides of its neck.

As the quantities of heat given off are supposed to have been as the quantities of surface exposed to the air, if we suppose the whole quantity of heat lost by the instrument to be = 10000 parts, the quantity which passed through the vertical sides of the instrument in $45\frac{1}{2}$ minutes, in the experiment, must have amounted to 5885 parts. For, the whole of the surface of the instrument, = 85.195 superficial inches, is to the whole of the heat given off, = 10000, as the surface of the vertical sides of the instrument, = 50.136 superficial inches, to the quantity of heat which must have passed off through that surface in the given time, = 5885.

Now, as we may with safety conclude, that the quantity of

heat which passes off through a *given surface* must be as the times elapsed, all other circumstances being the same, we can determine how much of the heat given off by the instrument, in those experiments in which its ends were covered, passed through the sides of the instrument; and, consequently, how much of it must have made its way through its ends and neck, notwithstanding their being covered.

The instrument with its ends and neck covered up with eider-down, furs, &c. was found to cool through the standard interval of 10 degrees in $55\frac{1}{2}$ minutes. Now, as only 5885 parts of heat were found to pass through the naked vertical sides of the instrument in $45\frac{1}{2}$ minutes, no more than 7015 parts could have passed through the same surface in $55\frac{1}{2}$ minutes; consequently, the remainder of the heat lost by the instrument, in the experiment in question, amounting to 2985 parts, must necessarily have made its way through the covered ends and neck of the instrument, in the given period, $55\frac{1}{2}$ minutes.

Taking it for granted that these computations are well founded, we may now proceed to a more exact determination of the relative quantities of heat which made their way through the sides of the instrument No. 2, when its sides were exposed naked to the air, and when they were covered with the different substances which appeared to facilitate the escape of the heat.

In the experiment No. 11, when the sides of the instrument were made quite black, by holding it over the flame of a wax candle, the instrument cooled through the standard interval of 10 degrees in $36\frac{1}{6}$ minutes.

In that time, a quantity of heat = 1949 parts, must have passed off through the covered ends and neck of the instrument; for, if a quantity = 2985 parts could pass off that way in $55\frac{1}{2}$

minutes, the quantity above-mentioned (= 1942 parts) must have escaped in $36\frac{1}{8}$ minutes.

This quantity, = 1942 parts, taken from the whole quantity, = 10000 parts, lost by the instrument in cooling through the interval in question, leaves 8058 parts, for the quantity which made its escape through the sides of the instrument, in the experiment in question.

Now, if a quantity of heat = 7015 parts, requires $55\frac{1}{2}$ minutes to make its way through the naked sides of the instrument, (as we have just seen,) it would require $63\frac{3}{4}$ minutes, for the quantity in question, = 8058 parts, to pass off through the same surface.

But, when that surface was blackened over the flame of a candle, that quantity of heat passed off through it in $36\frac{1}{8}$ minutes.

Hence it appears, that the velocity with which heat is given off from the naked surface of a heated metal exposed to cool in the air, is to the velocity with which it is given off by the same metal when its surface is blackened in the manner above described, as $36\frac{1}{8}$ to $63\frac{3}{4}$, or as 5654 to 10000, very nearly; for the velocities are as the times of cooling, inversely.

Again, in the experiment No. 6, the sides of the instrument No. 2 being covered with four coatings of spirit varnish, the instrument was found to cool through the given interval of 10 degrees in $30\frac{1}{4}$ minutes,

In that time, a quantity of heat = 1627 parts, must have made its way through the covered ends of the instrument; and the remainder, = 8373 parts, must have made its way through its varnished sides.

This quantity, = 8373 parts, would have required $66\frac{1}{4}$ minutes, to have made its way through the naked sides of the instrument; and, as it actually made its way through the varnished sides of

the instrument in 30¼ minutes, it appears that the velocity with which the heat was given off from the naked metallic surface, was to the velocity with which it was given off from the same surface covered with four coatings of spirit varnish, as 66¼ to 30¼, or as 10000 to 4566.

Without pursuing these computations any farther, at present, and without stopping to make any remarks on the curious facts they present to us, I shall hasten to experiments from the results of which we shall obtain more satisfactory information. But, before I proceed any farther, I must give an account of an instrument I contrived for measuring, or rather for *discovering*, those very small changes of temperature in bodies, which are occasioned by the radiations of other neighbouring bodies, which happen to be at a higher, or at a lower temperature.

This instrument, which I shall take the liberty to call a *thermoscope*, is very simple in its construction. Like the hygrometer of Mr. LESLIE, (as he has chosen to call his instrument,) it is composed of two glass balls, attached to the two ends of a bent glass tube; but the balls, instead of being near together, are placed at a considerable distance from each other; and the tube which connects them, instead of being bent in its middle, and its two extremities turned upwards, is quite straight in the middle, and its two extremities, to which its two balls are attached, are turned perpendicularly upwards, so as to form each a right angle with the middle part of the tube, which remains in a horizontal position.

At one of the elbows of this tube, there is inserted a short tube, of nearly the same diameter, by means of which, a very small quantity of spirit of wine, tinged of a red colour, is introduced into the instrument; and, after this is done, the end of

this short tube (which is only about an inch long) is sealed hermetically; and all communication is cut off, between the air in the balls of the instrument and in its tube, and the external air of the atmosphere.

A small *bubble* of the spirit of wine (if I may be allowed to use that expression) is now made to pass out of the short tube, into the long connecting tube; and the operation is so managed, that this bubble (which is about $\frac{3}{4}$ of an inch in length) remains stationary, at or near the middle of the horizontal part of the tube, *when the temperature (and consequently the elasticity) of the air in the two balls, at the two extremities of the tube, is precisely the same.*

By means of a scale of equal parts, attached to the horizontal part of the connecting tube, the position of the bubble can be ascertained, and its movements observed.

If now, the bubble being at rest in its proper place, one of the balls of the instrument be exposed to the calorific rays which proceed in all directions from a hot body, while the other ball is defended from those rays by a screen, the air in the ball so exposed to the action of these rays, will be heated; and, its elasticity being increased by this additional heat, its pressure will no longer be counterbalanced by the elasticity of the colder air in the other ball, and the bubble will be forced to move out of its place, and to take its station nearer to the colder ball.

By presenting two hot bodies, at the same time, to the two balls of the instrument, taking care that each ball shall be defended from the action of the hot body presented to the opposite ball, the distances of these hot bodies from their respective balls may be so regulated, that their actions on those balls may be equal, however the temperatures of those hot bodies may differ,

or however different may be the quantities, or intensities, of the calorific rays which they emit.

The instrument will show, with the greatest certainty, when the actions of these hot bodies on their respective balls are equal; for, until they become *unequal*, the bubble will remain immoveable in its place.

And, when the actions of two hot bodies on the instrument are equal, the relative intensities of the rays they emit may be ascertained, by the distances of the bodies from the balls of the instrument.

If their distances from their respective balls are equal, the intensities of the rays they emit must of course be equal.

If those distances are unequal, the intensities will probably be as the squares of the distances, inversely.

A distinct and satisfactory idea may be formed, of the instrument I have been describing, from Figure 2.

AB is a board, 27 inches long, 9 inches wide, and 1 inch thick, which serves as a support for the bent tube CDE, at the two extremities of which the two balls are fixed. The two projecting ends of the tube, C and E, which are in a vertical position, are each 10 inches long; and the horizontal part D of the tube, which is fastened down on the board, is 17 inches in length.

The balls are each 1.625 inches in diameter. The diameter of the tube is such, that 1 inch of it in length would contain 15 grains Troy of mercury.

The pillar F, which, by means of a horizontal arm projecting from it, serves for supporting the circular vertical screen represented in the figure, is firmly fixed in the board AB.

This circular screen (which is made of pasteboard, covered

on both sides with gilt paper) serves for preventing one of the balls of the instrument from being affected by the calorific rays proceeding from a hot body which is presented to the opposite ball.

Besides the circular screen represented in the figure, several other screens are used in making experiments; for the instrument is so extremely sensible, that the naked hand presented to one of the balls, at the distance of several inches, puts the bubble in motion; and it is affected very sensibly by the rays which proceed from the person who approaches it to make the experiments, unless care be taken, by the interposition of screens, to prevent those rays from falling on the balls. These screens can be best and most readily made, by providing light wooden frames, about 2 feet square, and half an inch in thickness, and covering them on both sides, first with thick cartridge paper, and then with what is called gilt paper; the metallic substance (copper) with which one side of the paper is covered being on the outside.

To support a moveable screen of this kind in a vertical position, it must of course be provided with a foot or stand. Those I use, are fastened to one side of a pillar of wood, by two screws; one of which passes through the centre of the screen, where the cross bars belonging to the frame of the screen meet; and the other through the middle of the piece of wood which forms the bottom of the screen. This pillar of wood, which is turned in a lathe, is $12\frac{1}{2}$ inches high, and is firmly fixed, at its lower end, in a piece of wood, 8 inches square, and 1 inch thick, which serves as a stand or foot, for supporting it.

As, in making experiments with this *thermoscope*, it is frequently necessary to remove the hot bodies, that are presented

to it, farther from it, or to bring them nearer to it; in order that this may be done easily, and expeditiously, by one person, and without its being necessary for him to remove his eye from the bubble, (which he should constantly have in his view,) I make use of a simple machine, which I have found to be very useful.

It is a long and shallow wooden box, open at both ends. It is 6 feet long, 12 inches wide, and 5 inches deep, measured on the outside: its vertical sides are made of $1\frac{1}{2}$-inch deal; its bottom and top, of inch deal. A part only of the top or cover of this box is fixed down on the sides, and is immoveable. The part of the cover which is fixed, and on which the thermoscope is placed, occupies the middle of the box, and is 19 inches in length. On the right and left of this fixed part, the top of the box is covered by a sliding board, 2 feet 3 inches long, which passes in deep grooves, made to receive it, in the sides of the box. A rack is fixed to the under side of each of these sliding boards; and there is a small cog wheel in the box, the axis of which passes through the sides of the box, and is furnished with a winch in the front of the box. By turning round these wheels, by means of their winches, (both of which can be managed by the same person, at the same time,) the sliders may be moved backwards and forwards, at pleasure.

In order to ascertain with facility and dispatch, the distances of the hot bodies from their respective balls, the top of the front side of the wooden box is divided into inches, on each side of the fixed part of the cover of the box: and there is a *nonius* belonging to each of the sliders, which is placed in such a manner as to indicate, at all times, the exact distance of the hot body from its corresponding ball.

The level of the upper surface of that part of the cover

which is fixed, is about $\frac{1}{4}$ of an inch higher than the level of the upper surface of the sliders: in order that, when a thermoscope longer than this fixed part is placed on it, the sliders may pass freely under its two projecting ends, without deranging it.

It is evident, from this description, that, by placing the thermoscope on the fixed part of the cover of the box, with its two balls in a line parallel to the axis of the box, and by placing the two hot bodies presented to the two balls of the instrument (elevated to a proper height) on stands set down on the sliders, an observer, by taking the two winches in his hands, keeping his eye fixed on the bubble, may, with the greatest facility, so regulate the distances of the hot bodies from their respective balls, that the bubble shall remain immoveable in its place.

In order to be able to ascertain precisely the temperatures of the hot bodies presented to this instrument, and in order that their surfaces might be equal, two equal cylindrical vessels, of thin sheet brass, with oblique cylindrical necks, were provided, of the form represented in Figure 3.

This cylindrical vessel, which is placed in a horizontal position, in order that its flat bottom may be presented, *in a vertical position*, to one of the balls of the thermoscope, is so fixed to a wooden stand, of a peculiar construction, that it may be raised, or lowered, at pleasure. This is necessary, in order that its axis may be in the continuation of a line passing through the centres of the two balls of the thermoscope.

This cylindrical vessel is 3 inches in diameter, and 4 inches in length; and its oblique cylindrical neck is 0.86 of an inch in diameter, and 3.8 inches in length.

The neck of this vessel is inserted *obliquely* into its cylindrical

body, in order that the water with which it is occasionally filled may not run out of it, when the body of the vessel is laid down in a horizontal position, in the manner represented in the above-mentioned figure.

A thermometer, with a cylindrical bulb 4 inches in length, being inserted into the body of this vessel, through its neck, shows the temperature of the contained water.

Care is necessary, in constructing a thermoscope, to choose a tube of a proper diameter: if its bore be too small, it will be found very difficult to keep the spirit of wine in one mass; and, if it be too large, the little horizontal column it forms, (which I have called a bubble,) will be ill defined at its two ends, which will render it difficult to ascertain its precise situation. After a number of trials, I have found, that a tube, the bore of which is of such a size that 1 inch of it in length contains about 15 or 18 grains Troy of mercury, answers best. For a tube of that size, the balls may be about 1½ inch in diameter; and they should both be painted black, with Indian ink, which renders the instrument more sensible.

I have an instrument of this kind, the tube of which is quite filled with spirit of wine, excepting only the space occupied by a small bubble of air, which is introduced into the middle of the horizontal part of the tube; but it does not answer so well as those which contain only a very small quantity of that liquid, sufficient to form a small bubble.

But, without enlarging any farther, at present, on the construction of these instruments, I now proceed to give an account of the experiments for which they were contrived.

Having found abundant reason to conclude, from the results of the experiments of which an account has already been given,

that all the heat which a hot body loses, when it is exposed in the air to cool, is not given off to the air which comes into contact with it; but that a large proportion of it escapes in rays, which do not heat the transparent air through which they pass, but, like light, generate heat only when, and where, they are stopped and absorbed: I suspected that, in every case when, in the foregoing experiments, the cooling of my instruments was expedited by coverings applied to their metallic surfaces, those coverings must, by some means or other, have facilitated and accelerated the emission of calorific rays from the hot surface.

Those suspicions implied, it is true, the supposition that different substances, heated to the same temperature, emit unequal quantities of calorific rays; but I saw no reason why this might not be the case in fact; and I hastened to make the following experiments, which put the matter beyond all doubt.

Exper. No. 12. Two equal cylindrical vessels, made of sheet brass, and polished very bright, each 3 inches in diameter, and 4 inches long, suspended by their oblique necks, in a horizontal position, (being placed on their wooden stands,) were filled with water at the temperature of 180°; and their circular flat bottoms were presented, in a vertical position, to the two balls of the thermoscope, at the distance of 2 inches.

When the two hot bodies were presented, at the same moment, to the two balls of the instrument, or, what was still better, when two screens were placed before the two balls, at the distance of about an inch, and, after the hot bodies were placed, these screens were both removed at the same instant, the small column of spirit of wine, which I have called a *bubble*, remained immoveable in its place, in the middle of the horizontal part of the tube of the instrument.

If one of the hot bodies was now brought nearer the ball to which it was presented, (the other hot body remaining in its place,) the bubble immediately began to move from the hot body which was advanced forward, towards the opposite ball, to which the other hot body was presented.

If, instead of advancing one of the hot bodies nearer the ball to which it was presented, it was drawn backward to a greater distance from it, the action of its calorific rays on the ball was diminished by this increase of distance; and, being overcome by the action of the rays from the hot body presented to the opposite ball, (at a smaller distance,) the bubble was forced out of its place, and obliged to move towards the ball which had been drawn backward.

When one of the hot bodies only was presented to one of the balls, the bubble was immediately put in motion; and, by bringing the hot body nearer to the ball, it might be driven quite out of the tube, into the opposite ball; this, however, should never be done, because it totally deranges the instrument, as it is easy to perceive it must do.

Having, by these trials, ascertained the sensibility and the accuracy of my instrument, I now proceeded to make the following decisive experiment.

Exper. No. 13. Having blackened the flat circular bottom of one of the cylindrical vessels, by holding it over the flame of a wax candle, I now filled both vessels again with water at the temperature of 180° F. and presented them, as before, to the two opposite balls of the instrument, at equal distances.

The bubble was instantly driven out of its place, by the superior action of the blackened surface; and did not return to its former station, till after the vessel which was blackened

had been removed to more than 8 inches from the ball to which it was presented; the other vessel, which had not been blackened, remaining in its former situation, at the distance of 3 inches from its ball.

The result of this experiment appeared to me to throw a new light on the subject which had so long engaged my attention; and to present a wide and very interesting field for farther investigation.

I could now account, in a manner somewhat more satisfactory, for those appearances in the foregoing experiments which were so difficult to explain,—for the acceleration of the passage of the heat out of my instruments, which resulted from covering them with linen, varnish, &c. and I immediately set about making a variety of new experiments, from which I conceived I should acquire a farther insight into those invisible mechanical operations which take place when bodies are heated and cooled.

Finding so great a difference in the quantities of calorific rays which are thrown off by the polished surface of a metal, when exposed *naked* to the cold air, and when *blackened*, I now proceeded to make experiments, to ascertain whether or not all those substances with which the sides of my cylindrical vessels had been covered, and which had been found to expedite the cooling of those instruments, would also facilitate the emission of calorific rays from the surfaces of the instruments I presented to the balls of my thermoscope; and I found this to be the case in fact.

As the results of all these experiments proved, in the most decisive manner, that all the substances which, when applied to the metallic surfaces of my large cylindrical vessels, had expedited their cooling, facilitated and expedited the emission of

calorific rays, I could no longer entertain any doubts respecting the agency of *radiation*, in the heating and cooling of bodies. Many important points however still remained to be investigated, before distinct and satisfactory ideas could be formed, respecting the nature of those rays, and the mode of their action.

I had hitherto made use of but one metal (brass) in my experiments; and that was not a simple, but a compound metal. The first subject of enquiry which presented itself, in the prosecution of these researches, was to find out whether or not similar experiments made with other metals would give similar results.

Exper. No. 14. Procuring from a gold-beater a quantity of leaf gold and leaf silver, about three times as thick as that which is commonly used by gilders, I covered the surfaces of the two large cylindrical vessels, No. 1 and No. 2, with a single coating of oil varnish; and, when it was sufficiently dry for my purpose, I gilt the instrument No. 1 with the gold leaf, and covered the other, No. 2, with silver leaf. When the varnish was perfectly dry and hard, I wiped the instruments with cotton, to remove the superfluous particles of the gold and silver, and then repeated the experiment so often mentioned, of filling the instruments with boiling hot water, and exposing them to cool, in the air of a large quiet room.

The time of cooling through the given interval of 10 degrees, was just the same as it was before, when the natural surface of these brass vessels was exposed *naked* to the air. I repeated the experiment several times, but could not find that the difference in the metals made any difference in the times of cooling.

Exper. No. 15. Not satisfied to rest the determination of so important a point on a trial with three metals only, brass, gold,

and silver, I now provided myself with two new instruments, the one made of lead, and the other covered with tinned sheet iron, improperly, in England, called tin.

As the *conducting power* of lead, with respect to heat, is much greater than that of any other metal, I conceived that, if the *radiation* of a body were any way connected with its *conducting power*, the cooling of the water contained in the leaden vessel, would necessarily be either more or less rapid than in a vessel constructed of any other metal.

The result of this experiment, as also the results of several others similar to it, showed that heat is given off with the same facility, or with the same celerity, from the surfaces of all the metals.

Is not this owing to their being all equally wanting in *transparency?* And does not this afford us a strong presumption that heat is, in all cases, excited and communicated by means of radiations, or *undulations*, as I should rather choose to call them?

I am sensible, however, that there is another and most important question to be decided, before these points can be determined; and that is, whether bodies are cooled in consequence of the rays they emit, or by those they receive?

The celebrated experiment of Professor PICTET, which has often been repeated, appears to me to have put the fact beyond all doubt, that rays, or emanations, which, like light, may be concentrated by concave mirrors, proceed from cold bodies; and that these rays, when so concentrated, are capable of affecting, in a manner perfectly sensible, a delicate air thermometer.

One of the objects I had principally in view, in contriving the

before described instrument, which I have called a thermoscope, was to investigate the nature and properties of those emanations; and to find out, if possible, whether they are not of the same nature as those calorific rays which have long been known to proceed from hot bodies.

My first attempts, in these investigations, were to ascertain the existence of those emanations universally; and to discover what visible effects they might be made to produce, independently of concentration by means of concave mirrors.

Exper. No. 16. My two horizontal cylindrical vessels, of sheet brass, (of the same form and dimensions,) having been made very clean and bright, were fixed to their stands; and, being elevated to a proper height to be presented to the balls of the thermoscope, were set down near that instrument, (which was placed on a table in a large quiet room,) where they were suffered to remain several hours, in order that the whole of this apparatus might acquire precisely the same temperature.

Day-light was excluded, by closing the window-shutters; and, in order that the thermoscope might not be deranged by the calorific rays proceeding from the person of the observer, on his entering the room to complete the intended experiments, screens were previously placed before the instrument, in such a manner that its balls were completely defended from those rays.

Things having been thus prepared, I entered the room as gently as possible, in order not to put the air of the room in motion, and, approaching the thermoscope, presented first one, and then the other cylindrical vessel, to one of the balls of the instrument; but it was not in the least degree affected by them,

the bubble of spirit of wine remaining immoveably in the same place.

Exper. No. 17. Having assured myself, by these previous trials, that the instrument was not sensibly affected by a bright metallic surface being presented to it, provided the temperature of the metal and that of the instrument were the same, I now withdrew one of the cylindrical vessels, and, taking it into another room, I filled it with pounded ice and water.

Entering the room again, I now presented the flat vertical bottom of this horizontal cylindrical vessel, filled with ice and water, to one of the balls of the thermoscope, at the distance of four inches.

The bubble of spirit of wine began instantly to move, with a slow regular motion, towards the cold body; and, having advanced in the tube about an inch, it remained stationary.

On bringing the cold body nearer the ball to which it was presented, the bubble was again put in motion, and advanced still farther towards the cold body.

Exper. No. 18. Although the result of the foregoing experiment appeared to me to afford the most indisputable proof of the *radiation* of cold bodies, and that the rays which proceed from them have a power of *generating cold* in warmer bodies which are exposed to their influence, yet, in a matter so extremely curious, and of such high importance to the science of heat, I was not willing to rest my enquiries on the result of a single experiment.

In order to vary the substance, or species of matter, presented cold to the instrument, and, at the same time, to remove all suspicion respecting the possibility of the effects observed being

produced by currents of cold air occasioned in the room by the presence of the cold body, I now repeated the experiment with the following variations.

The thermoscope was laid down on one side, so that the two ends of its tube, to which its balls were attached, instead of being vertical, were now in a horizontal position; and the cold body, instead of being presented to the ball of the instrument on one side of it, and on the same horizontal level with it, was now placed *directly under it*, and at the distance of 6 inches.

This cold body, instead of being a metallic substance, was a solid cake of ice, circular, flat, and about 3 inches thick, and 8 inches in diameter. It was placed in a shallow earthen dish, about 9 inches in diameter below, 12 inches in diameter above, at its brim, and 4 inches deep. The cake of ice being laid down on the bottom of the dish, the top of the dish was covered by a circular piece of thick paper, 14 inches in diameter, which had a circular hole in its centre, just 6 inches in diameter.

This earthen dish, containing the ice, and thus covered, was placed perpendicularly under one of the balls of the thermoscope, at such a distance that the centre of the upper surface of the flat cake of ice was 6 inches below the ball.

The result of this experiment was just what might have been expected: the ice was no sooner placed under the ball of the instrument, than the bubble of spirit of wine began to move towards that side where the cold body was placed; and it did not remain stationary, till after it had advanced more than an inch in the tube.

Exper. No. 19. Desirous of discovering whether the surface of a liquid emits frigorific or calorific rays, as solid bodies have been found to do, I now removed the cake of ice from the

earthen dish, and replaced it with an equal mass of ice-cold water.

The result of this experiment was, to all appearance, just the same as that of the last. The bubble moved towards the cold body, and took its station in the same place where it had remained stationary before. I found reason however to conclude, after meditating on the subject, that although the last experiment proves, in a most decisive manner, that radiations actually proceed from the surface of *water*, yet the proof of the radiation from the surface of ice, afforded by the preceding experiment, is not equally conclusive; for, as the temperature of the air of the room in which these experiments were made, was many degrees above the freezing point, it is possible, and even probable, that the surface of the ice was actually covered with a very thin, and consequently invisible, coating of water, during the whole of the time the experiment lasted.

Finding reason to conclude, that frigorific rays are always emitted by cold bodies, and that these emanations are very analogous to the calorific rays which hot bodies emit, I was impatient to discover, whether all cold bodies, at the same temperature, emit the same quantity of rays, or whether (as I had found to be the case with respect to the calorific rays emitted by hot bodies) some substances emit more of them, and some less.

With a view to the ascertaining of this important point, I made the following experiments.

Exper. No. 20. Having found that a metallic surface, rendered quite black by holding it over the flame of a wax candle, emits a much larger quantity of calorific rays, when hot, than the same metal, at the same temperature, throws off when naked, I was very curious to find out whether blackening the surface of

a cold metal, would or would not increase, in like manner, the quantity of frigorific rays emitted by it.

Having blackened, in the manner already described, the flat bottom, or rather end, of one of my horizontal cylindrical brass vessels with an oblique neck, I filled it with a mixture of ice and common salt; and, filling another vessel of the same kind, the bottom of which was not blackened, with the same cold mixture, I presented them both, at the same instant, and at the same distance, to the two opposite balls of my thermoscope.

The result of this experiment was perfectly conclusive: the bubble of spirit of wine began immediately to move towards the ball to which the *blackened* cold body was presented; indicating thereby, that that ball was more cooled by the frigorific rays which proceeded from the blackened surface, than the opposite ball was cooled by the rays which proceeded from an equal surface of naked metal, at the same temperature.

As this experiment appeared to me to be of great importance, I repeated it several times, and always with the same results; the motion of the bubble, which constituted the index of the instrument, constantly showing that the frigorific rays from the blackened surface were more powerful, in generating cold, than those which proceeded from the naked metal.

The bubble, it is true, did not move so far out of its place as it had done in the experiments in which hot bodies were presented to the balls; but this was not to be expected; for, though I had taken pains, by mixing salt with the ice, to produce as great a degree of cold as I conveniently could, yet still, the difference between the temperature of the balls and that of the bodies presented to them, was much greater when the hot bodies were used, than when the experiments were made with the cold bodies;

and it is evident, that the distance to which the bubble is driven out of its place, must necessarily be greater or less, in proportion as that difference is greater or less.

In those experiments in which the horizontal cylindrical vessels were filled with hot water, and then presented to the balls of the instrument, the temperature of the circular flat surfaces was that of 180°, while the temperature of the air of the room in which those experiments were made, and consequently that of the balls, was about 60°; the difference amounts to no less than 120 degrees of FAHRENHEIT's scale; but, in these experiments with cold, the difference of the temperatures at the moment when the cold bodies were first presented to the instrument, did not probably amount to more than 40, or at the most 50 degrees; and, in a very few seconds, it must have been reduced to less than 30 degrees, in consequence of the freezing of the water precipitated by the air of the atmosphere, on the surface of the vessel containing the cold mixture.

This precipitation of water, by the surrounding air, was so copious, that the brilliancy of the polish of the metallic surface was almost instantly obscured by it; and the vessels were very soon covered with a thick coat of ice. These accidents, which were not to be prevented, affected in a very sensible manner the results of the experiment. The bubble, instead of remaining stationary for some time after it had reached the point of its greatest elongation, as it had done in the experiments with hot bodies, had no sooner reached that point, than it began to return back towards the place from which it had set out; and, as often as I wiped off the ice from the surface of the flat end of the vessel which was not blackened, and presented it clean and bright to the ball of the instrument, the bubble began

again to move towards the opposite side; which, by the bye, shows that ice emits a greater quantity of frigorific rays than a bright metallic surface, at the same temperature.

Having frequently observed, on presenting my hand to one of the balls of the thermoscope, that the instrument was greatly affected by the calorific rays which proceeded from it, apparently much more so than it would have been by a much hotter body, of the same quantity of surface, but of a different kind of substance, placed at the same distance, I was extremely curious to find out, whether *animal substances* do not emit calorific (and consequently frigorific) rays much more copiously than other substances; and whether living animal bodies do not emit them in greater abundance than dead animal matter.

The first experiment I made, with a view to the investigation of this particular point, was as simple as its result was striking and conclusive.

Exper. No. 21. Having procured a piece of gold-beater's skin, (which, as is well known, is one of the membranes that line the larger intestines in cattle, and is exceedingly thin,) I moistened it with water; and, applying it, while moist, to the flat circular end of one of my horizontal cylindrical vessels, it remained firmly attached to the surface of the metal, when it became dry. I now filled this vessel, and another, of equal dimensions, the end of which was not covered, with hot water, (at the temperature of 180°,) and presented them both, at the same moment, to the two balls of the thermoscope, and at the same distance.

The bubble of spirit of wine was immediately driven out of its place, to a great distance; and did not return to its former station, till after the vessel whose end was covered with gold-

beater's skin had been removed to a distance from the ball to which it was presented, which was *five times* greater than the distance at which the other vessel was placed from the opposite ball.

I was induced to conclude, from the result of this interesting experiment, that an animal substance emits 25 *times* more calorific rays, than a polished metallic surface of the same dimensions, both substances being at the same temperature.

Exper. No. 22. Having emptied both the vessels used in the last experiment, and refilled them with pounded ice and water, I now presented them again to the thermoscope, at equal distances from their respective balls.

The result of this experiment confirmed the conclusion I had been induced to draw from a former experiment of the same kind, (No. 19,) the motion of the bubble towards the vessel whose surface was covered with gold-beater's skin, showing that the rays which proceeded from that animal substance were considerably more efficacious in producing cold, than those which proceeded from the naked metal.

The radiation of cold bodies appearing to me to have been proved beyond all doubt, by the preceding experiments, I now set about to investigate a very important point, which still remained to be determined: I endeavoured to find out, whether the intensity of the action of the frigorific rays which proceed from cold bodies, or their power of affecting the temperatures of other warmer bodies, *at equal intervals of temperature*, is, or is not, equal to the intensity of the action of the calorific rays which proceed from hot bodies. To ascertain this point, I made the following very simple and decisive experiment.

Exper. No. 23. Having placed the thermoscope on a table, in

the middle of a large quiet room, at the temperature of 72° F. I presented to one of its balls, at the distance of 3 inches, the flat circular end of one of the horizontal cylindrical vessels (A) above described, with an oblique cylindrical neck, this vessel being filled with pounded ice and water; and, at the same moment, an assistant presented to the opposite side of the same ball of the thermoscope, at the same distance, (3 inches,) the flat end of the other similar and equal cylindrical vessel, (B,) filled with warm water at the temperature of 112° F. the opposite ball of the thermoscope being hid and defended, by means of screens, from the actions of the bodies presented to the other ball, as also from the calorific rays which proceeded from the bodies of the persons present.

From this description it appears, that while one of the balls of the thermoscope was so defended by screens that it could not be sensibly affected by the radiations of the neighbouring bodies, the other ball was exposed to the simultaneous action of two equal bodies, at equal distances; (two vertical metallic disks, 3 inches in diameter, placed on opposite sides of the ball, at the distance of 3 inches;) one of these bodies being at the temperature of 32° F. or 40 degrees below that of the ball, while the other was at 112° F. or 40 degrees above the temperature of the ball.

I knew, from the results of former experiments, that this ball would, at the same time, be heated by the calorific rays from the hot body, and cooled by the frigorific rays from the cold body; and I concluded, that if its mean temperature should remain unchanged under the influence of these two opposite actions, that event would be a decisive proof of the equality of the intensities of those actions.

The result of the experiment showed, that the intensities of

those opposite actions were in fact equal; the bubble of spirit of wine, which, by its motion, would have indicated the smallest change of temperature in the ball of the thermoscope, to which the hot and the cold bodies were presented, remained at rest.

On removing the cold body a little farther from the ball, to the distance of $8\frac{1}{4}$ inches, for instance, the hot body remaining in its former station, at the distance of 8 inches, the bubble began immediately to move towards the opposite ball of the thermoscope, indicating an increase of heat in the ball exposed to the actions of the hot and the cold bodies; but, when the hot body was removed to a greater distance, the cold body remaining in its place, the bubble indicated an increase of cold.

The celerity with which the ball of the thermoscope acquired heat, or cold, might be estimated by the velocity with which the bubble of spirit of wine advanced, or retired, in its tube; but, on the most careful and attentive observation, I could not perceive that it moved faster when the ball was acquiring heat, than when it was acquiring cold; provided that the hot and the cold bodies, from which the calorific and frigorific rays proceeded, were at the same relative distances.

From these experiments, which I lately repeated at Geneva, in the presence of Professor PICTET, Mons. de SAUSSURE, M. SENEBIER, and several other persons, we may venture to conclude, that, *at equal intervals of temperature*, the rays which generate cold, are just as real, and just as intense, as those which generate heat; or, that their actions are equally powerful, in changing the temperatures of neighbouring bodies.

On a superficial view of this subject, it might appear extraordinary, that so important a fact as that of the frigorific radiations of cold bodies should have been so long unnoticed, while the

calorific radiations of hot bodies have been so well known; but, if we consider the matter with attention, our surprise will cease. Those radiations by means of which the temperatures of neighbouring bodies are gradually changed and equalized, are not sensible to our feeling, unless the intervals of temperature be very considerable; and the constitution of things is such, that while we are often exposed to the influence of bodies heated several thousand degrees (as measured by the thermometer) above the mean temperature of the surface of the skin, it is very seldom that we have opportunities of experiencing the effects of the radiations of bodies much colder than ourselves; and we have no means of producing degrees of cold which bear any proportion to the intense heats excited by means of fire.

From the result of the experiment of which an account has just been given, it is evident, that we should be just as much affected by the calorific rays emitted by a cannon bullet at the temperature of 160 degrees of FAHRENHEIT's scale, (=64 degrees above that of the blood,) as by the frigorific rays of an equal bullet, ice cold, placed at the same distance; and that a bullet at the temperature of freezing mercury, could not affect us much more sensibly, by its frigorific rays, than an equal bullet at the temperature of boiling water would do, by its calorific rays; but, at these comparatively small intervals of temperature, the radiations of bodies are hardly sensible, and could never have been perceived, much less compared and estimated, without the assistance of instruments much more delicate than our organs of feeling. Hence we see how it happened, that the frigorific radiations of cold bodies remained so long unknown. They were suspected by BACON; but their existence was first ascertained

by an experiment made at Florence, towards the end of the seventeenth century. And it is not a little curious, that the learned academicians who made that experiment, and who made it with a direct view to determine the fact in question, were so completely blinded by their prejudices respecting the nature of heat, that they did not believe the report of their own eyes; but, regarding the reflection and concentration of cold (which they considered as a negative quality) as *impossible*, they concluded, that the indication of such reflection and concentration, which they observed, must necessarily have arisen from some error committed in making the experiment.

Happily for the progress of science, the matter was again taken up, about twenty years ago, by Professor PICTET; and the interesting fact, which the Florentine academicians would not discover, was put beyond all doubt. But still, this ingenious and enlightened philosopher did not consider the appearances of a reflection of cold, which he observed in his experiments, as being *real*; nor was he led by them to admit the existence of frigorific emanations from cold bodies, analogous to those calorific emanations from hot bodies, which he calls radiant heat. He every where speaks of the reflection of cold (by metallic mirrors) as being merely *apparent*; and it is on that supposition, that the explanation he has given of the phenomena is founded.

On a supposition that the *caloric* of modern chemists has any real existence, and that heat, or an increase of temperature in any body, is caused by an *accumulation* of that substance in such body, the reflection of cold would indeed be impossible; and the supposition that such an event had taken place, would be absurd, and could not be admitted, however striking and convincing the

appearances might be which indicated that event. But, to return from this digression.

Having found that the intensity of the calorific rays emitted by a hot body, at any given temperature, depends much on the surface of such body,—that a polished metallic surface, for instance, throws off much fewer rays than the same surface, at the same temperature, would emit, if painted, or blackened in the smoke of a lamp or candle, I was desirous of finding out, whether the frigorific rays from cold bodies are affected in the same manner, by the same means, and in the same degree.

It was to ascertain that point, that the experiment No. 20 was made; and, although the result of that experiment afforded abundant reason to conclude, that those substances which, when hot, throw off calorific rays in the greatest abundance, actually throw off great quantities of frigorific rays, when they are cold; yet, as the relative quantities of these rays could not be exactly determined by that experiment, in order to ascertain so important a fact, I had recourse to the following simple contrivance.

Exper. No. 24. Having found, by the result of the last experiment, (No. 23,) that the calorific emanations of a circular disk of polished brass, 3 inches in diameter, at the temperature of 112° F. were just counterbalanced by the frigorific emanations of an equal disk of the same polished metal, at the temperature of 32° F. placed opposite to it, so that one of the balls of the thermoscope placed between these two disks, at equal distances, was just as much heated by the one as it was cooled by the other, I now blackened the two disks, by holding them over the flame of a wax candle, and repeated the experiment with them, so blackened.

I knew, from the results of former experiments, that the intensity of the calorific radiations from the hot disk, would be very much increased, in consequence of its surface being blackened; and I was certain, that if the intensity of the frigorific radiations of the cold disk should not be increased in *exactly the same degree*, the ball of the thermoscope, exposed to the simultaneous actions of these two disks, could not possibly remain at the same constant temperature, that of 72°.

The result of the experiment was very decisive: the bubble of spirit of wine remained at rest; which proved, that the intensities of the rays emitted by the two disks, still continued to be equal at the surface of the ball of the thermoscope, which, at equal distances, was exposed to their simultaneous action.

Hence we may conclude, that those circumstances which are favourable to the copious emission of calorific rays from the surfaces of hot bodies, are equally favourable to a copious emission of frigorific rays from similar bodies, when they are cold.

But it is time to consider these emanations in a new point of view. What difference can there be between calorific rays, and frigorific rays? Are not the same rays either calorific, or frigorific, according as the body at whose surface they arrive is hotter, or colder, than that from which they proceed?

Let us suppose three equal bodies, A, B, and C, (the globular bulbs of three mercurial thermometers, for instance,) to be placed, at equal distances, (3 inches,) in the same horizontal line; and let A be at the temperature of freezing water, B at the temperature of 72° F. and C at that of 109° F. The rays emitted by B will be *calorific*, in regard to the colder body A; but, in respect to the hotter body C, they will be frigorific; and,

from the results of the two last experiments, we have abundant reason to conclude, that they will be just as efficacious in heating the former, as in cooling the latter.

Before I proceed to give an account of the experiments which were made with a view to determine the relative quantities of rays emitted from the surfaces of various substances, from living animals, dead animal matter, &c. (which I must reserve for a future communication,) I shall lay before the Society the results of several experiments, of various kinds, which were made with a view to the farther investigation of the radiations of hot and of cold bodies, and of the effects produced by them.

Exper. No. 25. Having found, from the results of the experiments No. 21 and No. 22, that great quantities of rays are thrown off from the surface of the animal substance used in those experiments, (gold-beater's skin,) I now covered the whole of the external surface of one of my large cylindrical passage thermometers (No. 4) with that substance; and, filling it with boiling hot water, exposed it to cool gradually in the air of a large quiet room, in the manner often described in former parts of this Paper; another similar *naked* standard instrument (No. 3) being filled with hot water at the same time, and exposed to cool in the same situation.

The temperature of the air of the room being $51\frac{1}{2}°$, the instruments were found to cool through the standard interval of 10 degrees, namely, from $101\frac{1}{2}$ to $91\frac{1}{2}$, in the following times.

No. 4, *covered* with gold-beater's skin, in $97\frac{1}{4}$ minutes.

No. 3, which was *naked*, - - in 45 minutes.

Exper. No. 26. Being desirous of finding out whether or not the covering of animal matter, which had so remarkably facilitated the cooling of the instrument No. 4, would be equally

efficacious in facilitating the passage of heat *into* the instrument, I suffered both instruments to remain in the cold room all night; and, entering the next morning, at half an hour past seven o'clock, I found the temperature of the water in the *naked* instrument, No. 3, to be $50\frac{1}{4}°$: that in the instrument No. 4, which was covered with gold-beater's skin, was $49\frac{1}{4}°$; while the air of the room was at 48°.

At $7^h\ 30^m$ A. M. I removed both instruments into a warm room, and observed the times of their acquiring heat to be as expressed in the following Table.

Times when the observations were made.	Observed Temperature. No. 3, *naked*.	No. 4, *covered*.	Temperature of the air of the room.
At $7^h\ 30^m$	$50\frac{1}{4}°$	$49\frac{1}{4}°$	$64°$
7 45	$51\frac{1}{2}$	$51\frac{1}{2}$	$64\frac{1}{2}$
8 —	$52\frac{1}{2}$	$53\frac{1}{8}$	65
8 15	$53\frac{3}{4}$	$54\frac{7}{8}$	—
8 30	$54\frac{3}{8}$	56	—
8 45	$55\frac{1}{2}$	$57\frac{1}{8}$	—
9 —	$56\frac{1}{4}$	$58\frac{1}{2}$	—
9 30	$57\frac{1}{2}$	60	—
10 —	$58\frac{1}{4}$	$61\frac{1}{4}$	—
10 30	$59\frac{1}{2}$	$62\frac{1}{8}$	—
11 —	$60\frac{1}{2}$	63	—
11 30	61	$63\frac{1}{2}$	$64\frac{1}{2}$

The results of this experiment, and of several others similar to it, showed, in a manner which appeared to me to be perfectly conclusive, that those substances which part with heat with the greatest facility, or celerity, are those which also acquire it most readily, or with the greatest celerity.

If we might suppose that the temperatures of bodies are changed, not by the rays they *emit*, but by those they *receive* from other neighbouring bodies, this fact might easily be explained; but, without stopping to form any hypothesis for the explanation of these appearances, I shall proceed in my account of the various attempts I have made to elucidate, by new experiments, those parts of this interesting subject which still appeared to be enveloped in obscurity.

As the cooling of hot bodies is so much accelerated by covering their surfaces with such substances as emit calorific rays in great abundance, or with such as are much affected by the frigorific rays of the colder bodies by which they are surrounded, it seems to be highly probable, that a comparatively small part of the heat, which a body so cooled actually loses, is acquired by the air; a much greater proportion of it passing off through that *transparent* fluid, under the form of calorific rays, without affecting its temperature.

If this supposition should turn out to be well founded, the knowledge of the fact would enable us to explain several interesting phenomena, and particularly that most curious process by means of which living animals preserve an equal temperature, notwithstanding the vast quantities of heat that are continually generated in the lungs, and notwithstanding the great variations which take place in the temperature of the air in which they live.

It is evident, that the greater the power is which an animal possesses of *throwing off* heat from the surface of his body, independently of that which the surrounding air takes off, the less will his temperature be affected by the occasional changes of

temperature which take place in the air; and the less will he be oppressed by the intense heats of hot climates.

It is well known that *negroes*, and people of colour, support the heats of Tropical climates much better than white people. Is it not probable that their *colour* may enable them to throw off calorific rays with great facility, and in great abundance; and that it is to this circumstance they owe the advantage they possess over white people, in supporting heat? And, even should it be true, that bodies are cooled, not in consequence of the rays they emit, but by the action of those frigorific rays they receive from other colder bodies, (which I much suspect to be the case,) yet, as it has been found by experiment, that those bodies which emit calorific rays in the greatest abundance, are also most affected by the frigorific rays of colder bodies, it is evident, that in a very hot country, where the air and all other surrounding bodies are but very little colder than the surface of the skin, those who by their colour are prepared and disposed to be cooled with the greatest facility, will be the least likely to be oppressed by the accumulation of the heat generated in them by respiration, or of that excited by the sun's rays.

With a view to throw some light on this interesting subject, I made the following experiments.

Exper. No. 27. Having covered the flat ends of both my horizontal cylindrical vessels with gold-beater's skin, I painted one of these coverings (of this animal substance) black, with Indian ink; and then, filling both vessels with boiling hot water, I presented them, at equal distances, to the two opposite balls of the thermoscope.

The bubble of spirit of wine was immediately driven out of its

place, by the superior efficacy of the calorific rays which proceeded from the blackened animal substance.

On repeating this experiment a great number of times, and when the water in the vessels was at different degrees of temperature, (the temperature being the same in the two vessels, in each experiment,) the results uniformly indicated, that calorific rays were thrown off from the *black* surface, in greater abundance than from the equal surface which was not blackened.

Although the results of these experiments appeared to me to be so perfectly conclusive as to establish the fact in question, beyond all possibility of doubt, yet, in so interesting an enquiry, I was desirous, by varying my experiments, to bring, if possible, a variety of proofs, to support the important conclusions which result from it.

Exper. No. 28. Having covered the two large cylindrical vessels, No. 3 and No. 4, with gold-beater's skin, I painted one of them black, with Indian ink; and, filling them both with boiling hot water, I exposed them to cool, in the manner already often described, in the air of a quiet room.

No. 4, which was *blackened*, cooled through the standard interval of 10 degrees in 23½ minutes; while the other, No. 3, which was not blackened, took up 28 minutes, in cooling through the same interval.

In a former experiment, (No. 25,) the instrument No. 4, covered with gold-beater's skin, but not blackened, had taken up 27¼ minutes, in cooling through the given interval, as we have before seen.

The results of these experiments do not stand in need of illustration; and I shall leave to physicians and physiologists to determine what advantages may be derived from a knowledge of the

facts they establish, in taking measures for the preservation of the health of Europeans who quit their native climate to inhabit hot countries.

All I will venture to say on the subject is, that were I called to inhabit a very hot country, nothing should prevent me from making the experiment of blackening my skin, or at least of wearing a black shirt, in the shade, and especially at night; in order to find out if, by those means, I could not contrive to make myself more comfortable.

Several of the savage tribes which inhabit very cold countries, besmear their skins with oil; which gives them a shining appearance. The rays of light are reflected copiously from the surface of their bodies. May not the frigorific rays, which arrive at the surface of their skin, be also reflected, by the highly polished surface of the oil with which it is covered?

If that should be the case, instead of despising these poor creatures for their attachment to a useless and loathsome habit, we should be disposed to admire their ingenuity, or rather to admire and adore the goodness of their invisible guardian and instructor, who teaches them to like, and to practice, what he knows to be useful to them.

The Hottentots besmear themselves, and cover their bodies, in a manner still more disgusting. They think themselves *fine*, when they are besmeared and dressed out according to the loathsome custom of their country. But who knows whether they may not in fact be *more comfortable*, and better able to support the excessive heats to which they are exposed? From several experiments which I made, with a view to elucidate that point, (of which an account will be given to this Society at some future period,) I have been induced to conclude, that the Hottentots

derive advantages from that practice, exactly similar to those which negroes derive from their black colour.

It cannot surely be supposed, that I could ever think of recommending seriously to polished nations, the filthy practices of these savages. That is very far indeed from being my intention; for I have ever considered cleanliness as being so indispensably necessary to comfort and happiness, that we can have no real enjoyment without it; but still, I think that a knowledge of the physical advantages which those savages derive from such practices, may enable us to acquire the same advantages, by employing more elegant means. A knowledge of the manner in which heat and cold are excited, would enable us to take measures for these important purposes with perfect certainty: in the mean time, we may derive much useful information, by a careful examination of the phenomena which occasionally fall under our observation.

If it be true, that the black colour of a negro, by rendering him more sensible to the few frigorific rays which are to be found in a very hot country, enables him to support the great heats of Tropical climates without inconvenience, it might be asked, how it happens that he is able to support, naked, the direct rays of a burning sun?

Those who have seen negroes exposed naked to the sun's rays, in hot countries, must have observed that their skins, *in that situation*, are always very shining. An oil exudes from their skin, which gives it that shining appearance; and the polished surface of that oil reflects the sun's calorific rays.

If the heat be very intense, sweat makes its appearance at the surface of the skin. This watery fluid not only reflects very

powerfully the calorific rays from the sun, which fall on its polished surface, but also, by its evaporation, generates cold.

When the sun is gone down, the sweat disappears; the oil at the surface of the skin retires inwards; and the skin is left in a state very favourable to the admission of those feeble frigorific rays which arrive from the neighbouring objects.

But I shall refrain from pursuing these speculations any farther at present.

I shall now proceed to give an account of several experiments, of various kinds, which were made with a view to a farther investigation of the radiations of cold bodies.

Having found, by several of the foregoing experiments, that the radiations of cold bodies affected my thermoscope very sensibly, even when placed at a considerable distance from it, and in situations where currents of cold air could not be suspected to exist, I was desirous of finding out, whether the cooling of a hot body would or would not be *sensibly* accelerated by those rays. To determine that point, I made the following experiment.

Exper. No. 29. Having provided two conical vessels, made of thin sheet brass, each 4 inches in diameter at the base, and 4 inches high, ending above in a cylindrical neck, 0.88 of an inch in diameter, I enclosed each of them in a cylinder of thin pasteboard, covered with gilt paper, and then covered them up with rabbit-skins, which had the hair on them, in such a manner that no part of these vessels, except their flat bottoms, was exposed naked to the air. I then covered their bottoms with gold-beater's skin, painted black with Indian ink, in order to render them as sensible as possible to calorific and frigorific rays.

This being done, I suspended these two vessels, in an erect position, or with their bottoms downwards, to the two opposite horizontal arms of a wooden stand, provided for the experiment; and I placed under each of them a pewter platter, blackened on the inside, by holding it over a lighted wax candle.

Each of these platters was 12 inches in diameter; and they were supported on the top of two shallow earthen dishes, each of which was $11\frac{1}{2}$ inches in diameter at its brim; these earthen dishes being supported on circular wooden stands, 10 inches in diameter.

A circular piece of thick drawing-paper, $12\frac{1}{2}$ inches in diameter, with a circular hole in its centre, just 6 inches in diameter, was placed on each of the platters, and served as a perforated cover to it.

The stands on which the platters were supported, were of such a height, that the upper surface of the flat bottom of each of the platters was elevated just 40 inches above the level of the floor of the room; and the horizontal arms of the wooden stand, which supported the conical vessels, was of such a height, that the flat bottoms of these vessels (which were placed perpendicularly over the centres of the platters) were just 4 inches above the flat horizontal surface of the bottoms of the platters.

One of the platters was at the temperature of the air of the room, (63° F.) but the other was kept constantly ice-cold, during the whole of the time the experiment lasted, by means of pounded ice and water, which was put into the earthen dish, over which, or rather in which, this platter was placed.

Each of the platters was just 1 inch deep, measured from the level of the top of its brim to the level of the upper surface of

the flat part of its bottom: this flat part was about 8 inches in diameter.

The two conical vessels were now filled with boiling hot water, and the times of their cooling were carefully observed.

From the above description of the apparatus used in this experiment, it is evident, that the vessel which was suspended over the ice, could not be reached by any streams of cold air that might be occasioned by that ice, or by the cooled sides of the vessel which contained it; for the air which, coming into contact with the sides of that vessel, was cooled by it, becoming specifically heavier than it was before, naturally descended, and spread itself out on the floor of the room; and the perforated circular sheet of paper, which was laid down horizontally on the platter, effectually prevented any of the air so cooled from being thrown upwards against the bottom of the conical vessel, (placed immediately over the platter,) by any occasional undulation of the air in the room.

To preserve the air of the room in a state of perfect quietness, not only the doors and windows, but even the window-shutters of the room, were kept shut; so much light only being admitted occasionally, as was necessary to observe the thermometers which were placed in the conical vessels.

In order to guard still more effectually the bottoms of the vessels which were cooling, from the effects of occasional undulations in the air of the room, over each of these vessels there was drawn a cylindrical covering of very fine thin post paper; the lower open end of which projected just half an inch below the horizontal level of the flat bottom of the vessel. These cylindrical coverings of post paper were made to fit, as exactly as

possible, the cylinders of pasteboard by which the sides of the conical vessels were covered, and defended from the air; and the warm coverings of fur (rabbit-skins) were put over all.

To confine the heat still more effectually, a quantity of eider-down had been introduced between the outside of each conical vessel, and its cylindrical neck, and the inside of the hollow cylinder of pasteboard in the axis of which it was fixed and confined.

The result of this experiment was very conclusive. The conical vessel which was suspended over the *ice-cold* pewter platter, cooled through the standard interval of 10 degrees, (namely, from the point of 50 degrees to that of 40 degrees above the temperature of the air of the room,) in 33 minutes and 42 seconds; whereas, the other vessel, which was not over ice, required 89 minutes and 15 seconds, to cool through the same interval.

Exper. No. 30. On repeating this experiment the next day, the air of the room still remaining at 63°, the times of cooling through the given interval were as follows.

	Min.	Sec.
The vessel suspended over the ice-cold platter, in	33	15
The other vessel, in - - - - - -	89	30

From the results of these experiments (which were made with the greatest possible care) it appears, that the radiations of cold bodies act on warmer bodies, *at a distance*, and gradually diminish their temperatures.

It will likewise be evident, when we consider the matter with attention, that the cooling of the vessel which was suspended over the ice-cold platter, was in fact considerably more accelerated by the frigorific radiations from that cold surface than it appears to have been, when we estimate the effects produced

simply by the difference of the times taken up in the cooling of the two vessels, without having regard to any other circumstance.

These times are, no doubt, inversely as the velocities of cooling; but, as all the heat lost by the vessels, during the time of their cooling, did not pass off through their flat bottoms, and as the rays from the cold surface fell on the *bottom only* of the vessel which was suspended over it, without at all affecting its covered sides, the velocity with which the heat made its way through the covered sides of the vessels was the same in both; consequently, more heat must have passed that way, and of course less through the bottom of the vessel, when the time of cooling was the longest, that is to say, in the vessel which was not placed over ice.

As the cooling of these vessels is a complicated process, I will endeavour to elucidate the subject still farther.

As the two conical vessels were of the same form and dimensions, and contained equal quantities of hot water, the quantities of heat they parted with, in being cooled the same number of degrees, must of course have been equal.

Expressing that quantity by the algebraic symbol a, and putting $x =$ the quantity of heat which passed off through the covered sides of the vessel which was suspended over ice, during the time it was cooling through the given interval of 10 degrees, and $y =$ the quantity which passed off through the covered sides of the other vessel, during the time that vessel was cooling through the same interval; the quantity of heat which passed off through the bottom of the vessel which was placed over ice, during the time it was cooling through the given interval, must

have been $= a - x$; and that which passed off through the bottom of the other vessel, during the time of its cooling through the same interval, $= a - y$.

But, as the velocities of the heat through the covered sides of both vessels must have been equal, the quantities of heat which passed off *that way* must have been as the times of cooling.

The times of cooling in the last mentioned experiment, (No. 80,) were as follows:

	Min.	Sec.	Seconds.
Of the vessel suspended over ice	33	15	= 1995
Of the other vessel - -	39	30	= 2370

x is therefore to y, as 1995 to 2370;

consequently, $x = \frac{1995y}{2370} = 0.84177y$;

And, substituting for x, its value $= 0.84177y$, the quantities of heat which passed off through the bottoms of the two vessels, in the experiment in question, (No. 80,) must have been $= a - 0.84177y$, for the vessel which was suspended over ice; and $= a - y$, for the other vessel.

And, as y is greater than $0.84177y$, consequently $a - 0.84177y$ is greater than $a - y$, or the quantity of heat which passed off *through the bottom* of the vessel which was cooled the most rapidly, was greater than that which passed off *through the bottom* of the other vessel; and hence we perceive, that the effect produced by the frigorific rays from the cold surface, in the experiments in question, was *greater* than it appeared to be, at first sight, when it was estimated by the times of cooling.

To determine exactly *how much* the cooling was accelerated by the presence of the cold body, it is necessary to find out how much heat actually passed off through the bottoms of the two vessels, in the experiments in question. This we will endeavour

to do, by comparing the results of those experiments, with the results of some other experiments of a similar nature.

In the experiment No. 28, a cylindrical vessel of thin sheet brass, 4 inches in diameter, and 4 inches in height, covered with gold-beater's skin painted black with Indian ink, being filled with hot water, and exposed to cool in the air of a large quiet room, cooled from the point of 50 degrees to that of 40 degrees above the temperature of the air of the room, in $23\frac{1}{2}$ minutes.

The quantity of surface by which this vessel was exposed to the cold air, was = 74.5581 superficial inches, exclusive of its neck, which was well covered up with fur.

The quantity of surface which was exposed to the air, in the foregoing experiments with the conical vessels, or the area of the bottom of each of the vessels, was $(4 \times 3.14159) = 12.4263$ superficial inches.

As the diameters and heights of the conical and cylindrical vessels were equal, the contents of the former must have been to the contents of the latter as 1 to 3; and the quantities of heat which they lost in cooling were as their contents.

If now the cylindrical vessel lost a quantity of heat = 3, in $23\frac{1}{2}$ minutes, it would have disposed of a quantity = 1, (equal to that which the conical vessel lost,) in one-third part of that time, or in 7 minutes and 50 seconds.

But the quantity of surface exposed to the air in the experiment with the cylindrical vessel, was to that so exposed in the experiment with the conical vessel, as 74.5581 to 12.4263, or as 6 to 1.

Now, as the time in which any given quantity of heat can pass out of any closed vessel, into, or through, any cold fluid medium by which the vessel is surrounded, must be inversely as the

surface of the vessel, other things being equal; if a quantity of heat = 1 could pass out of the cylindrical vessel in 7 minutes and 50 seconds, it would require 6 times as long, or 47 minutes, to pass out of the conical vessel, *through its flat bottom*, supposing no heat whatever to escape through the covered sides of that vessel.

If now the whole of the heat which the conical vessel actually lost, would have required 47 minutes to have passed through the bottom of that vessel, it is evident that the quantity which actually passed through that surface, in the experiment in question, (No. 30,) could not have been, to the whole quantity actually lost, in a greater proportion than that of the times, or as $39\frac{1}{2}$ to 47.

Assuming any given number, as 10000, for instance, to represent the whole of the heat lost in the experiment, we can now determine what part or proportion of it passed off through the bottom of the conical vessel, and consequently how much of it must have made its way through its covered sides.

If the whole quantity, = 10000, would have required 47 minutes to have passed through the bottom of the vessel, the quantity which actually passed through that surface in $39\frac{1}{2}$ minutes, could not possibly have amounted to more than 8404, $= a - y$.

For it is 47 min. to 10000, as $39\frac{1}{2}$ min. to 8404. The remainder of the heat, = 10000 − 8404 = 1396 parts, (= y,) must have made its way through the covered sides of the vessel.

And, if a quantity of heat = 1396, required $39\frac{1}{2}$ minutes to make its way through the covered sides of one of the conical vessels, the quantity which made its way through the covered sides of the other in $33\frac{1}{4}$ minutes, could not have amounted to

more than 1175 parts; and the remainder of that which was actually disposed of in the experiment, $= 10000 - 1175 = 8825$, $(= a - x,)$ must have passed off through the bottom of the instrument.

Hence it appears, that the quantity of heat which actually passed off through the bottom of the conical vessel which was placed over ice, in $33\frac{1}{4}$ minutes, was to that which passed off in $39\frac{1}{3}$ minutes, through the bottom of the other vessel, as 8825 to 8404; and consequently, that the velocity with which the heat passed through the bottom of the vessel which was exposed to the frigorific rays from the surface of the cold platter, was to the velocity with which it passed through the bottom of the other vessel, in the compound ratio of 8825 to 8404, and of $39\frac{1}{3}$ to $33\frac{1}{4}$; or as 10000 to 8023, which is as 5 to 4, very nearly.

From these experiments and computations it appears, that the cooling of the hot body which was placed over the ice-cold platter, was sensibly, and very considerably, accelerated by the vicinity of that cold body; may we not venture to say,—by the frigorific rays which proceeded from it?

I made several other experiments, similar to those just described, and with similar results; but I shall not take up the time of the Society, by giving a detailed account of them. I may perhaps, at a future time, find occasion to mention some of them more particularly.

In the two last-mentioned experiments, as the conical vessels were suspended in an erect position, and had a circular band or hoop of fine post paper, by which the lower end of each of them was surrounded, and which projected downwards half an inch below the horizontal level of the bottom of the vessel, and as the air which came into immediate contact with the bottom of the

vessel, and received heat from it, (though it became specifically lighter than it was before,) could not make its escape *upwards*, into the atmosphere, being confined and prevented from moving upwards by the thin projecting hoop of paper, there is no doubt but that the time of cooling was prolonged by this arrangement; for, there being much reason to believe that the propagation of heat downwards, in air, from one particle of that fluid to another, is either quite impossible, or so extremely slow as to be imperceptible, as a succession of fresh particles of cold air was prevented from coming into contact with the bottoms of the vessels, but very little heat could have been given off *immediately* to the air in those experiments.

In order to be able to form some probable conjecture respecting the quantity so given off, in cases where the succession of fresh particles of air is free and uninterrupted, I made the following experiment.

Exper. No. 91. The two conical vessels used in the last experiment, (which I shall now distinguish by calling the one No. 5, and the other No. 6,) being left suspended in the air, to the two horizontal arms of their wooden stand, at the height of 44 inches above the floor of the room, (the pewter platters, the earthen dishes, and the stands on which they were placed being removed,) both the vessels were again filled with boiling hot water, and exposed to cool in the air.

The vessel No. 5 remained in a vertical position, or with its flat bottom in a horizontal position, as before; but the vessel No. 6 was now reclined, so that its axis, and consequently the plane of its flat bottom, made an angle with the plane of the horizon, of 45 degrees. In this position of the vessel No. 6, it is evident that the air, heated by coming into contact with its

bottom, had full liberty to escape *upwards*, and to make way for other particles of colder air to come into contact with the hot surface, and be heated, rarefied, and forced upwards, in their turns; and, under these circumstances, it might reasonably be expected, that as much heat as possible would be communicated *immediately* to the air, by the hot body; and that the heat so communicated would of course accelerate the cooling of that vessel.

It was in fact cooled in a shorter time than the other, No. 5, which was suspended in a vertical position; but the difference of the times of cooling was very small; which indicates, if I am not mistaken, that a comparatively small quantity of the heat a hot body loses, when it is cooled in air, is communicated to that fluid; much the greater part of it being sent off through the air, to a distance, in calorific rays.

The vessel No. 5 was found to cool through the standard interval of 10 degrees in 38¼ minutes; and No. 6, which was in a reclined position, in 37¼ minutes.

It will no doubt be remarked, that the vessel No. 5 cooled somewhat faster in this experiment than it had done in the two preceding experiments, (No. 29 and No. 30,) when it stood over a pewter platter, which (at the beginning of the experiment at least) was at the same temperature as the air of the room.

The calorific rays from the bottom of the vessel, heating the platter in some small degree, and still more perhaps the upper surface of the perforated sheet of paper which covered it, the frigorific rays from these bodies, were, on that account, somewhat less powerful in lowering the temperature of the neighbouring hot body; and the time of its cooling was consequently a little prolonged.

In one of the preceding experiments, it cooled through the given interval in $39\frac{1}{2}$ minutes, and in the other in $39\frac{1}{4}$ minutes; but, in this experiment, it took up only $38\frac{1}{2}$ minutes, in cooling through it, as we have just seen.

Supposing now, (what appears to me to be not improbable,) that all, or very nearly all, the heat lost by the instrument No. 5 passed off in rays *through* the air, we can ascertain what part of the heat lost by the instrument No. 6, was communicated *to the air* which came into contact with its surface.

Putting the total quantity of heat lost by each of the instruments, in cooling through the given interval, = 10000; as we have just seen that a quantity of heat = 1396, passes through the covered sides of each of these instruments in $39\frac{1}{2}$ minutes, the quantities so lost in this experiment must have been as follows. By the instrument No. 5, in $38\frac{1}{2}$ minutes, = 1081; by No. 6, in $37\frac{1}{4}$ minutes, = 1046; and, deducting these quantities so lost (through the covered sides of the instruments) from the total quantity lost by each, (= 10000,) we shall find out how much heat passed off *through the bottom* of each of the instruments.

For the instrument No. 5, it is $10000 - 1.081 = 9199$,

And for - - No. 6, $10000 - 1.046 = 9954$.

If now the whole of the heat lost through the bottom of the instrument No. 5, passed off *through* the air in rays, as there is no reason to suppose that a less quantity passed off in the same time, *in the same way*, through the bottom of the instrument No. 6, it appears, that this last mentioned instrument must have lost, *by radiation*, or in rays which passed *through* the air, a quantity of heat = 9597.

For it is $38\frac{1}{2}$ minutes to 9919, as $37\frac{1}{4}$ minutes to 9597.

And, if of the total quantity of heat which passed off through the bottom of the conical instrument No. 6, = 9954, a quantity =9597 passed off *through* the air, in calorific rays, the remainder only, (9954 − 9597,) which amounts to no more than 357 parts, could have been communicated to the air.

Hence it would appear, that when a hot body is cooled in air, $\frac{1}{27}$ part only of the heat which it loses is acquired by the air; for 357 is to 9597, as 1 to 27, very nearly. But I shall refrain from enlarging farther on this subject at present.

One of the objects which I had in view, in the last experiment, was, to find out whether the cooling of a hot body in air, is or is not sensibly accelerated, or retarded, by the greater or lesser distance at which the body is placed from other neighbouring solid bodies, when these neighbouring bodies are at the same temperature as the air; and, as a comparison of the result of this experiment, with the results of the two preceding experiments, so strongly indicated that the cooling of the conical vessel, in the preceding experiments, had in fact been *retarded* by the vicinity of the pewter platter over which it was suspended, I was now induced to repeat these experiments with some variations.

These investigations appeared to me to be of the more importance, as I conceived that the results of them might lead to a discovery of one of the causes of the warmth of clothing.

Exper. No. 32. I now placed the pewter platters once more in their former stations, perpendicularly under the bottoms of the two conical vessels, but at the distance of 3 inches only; that which was under the vessel No. 5 being at the temperature of the air of the room, (62°,) while that placed under the vessel

No. 6, was kept ice-cold, by means of pounded ice and water, which was put into the earthen dish on the brim of which it was supported.

The times of the cooling of the vessels, through the standard interval of 10 degrees, were as follows.

No. 5 - - - in 40¼ minutes.

No. 6, which was over ice, in 33¼

Exper. No. 33. I repeated this experiment once more, but varied it, by bringing the pewter platters still nearer to the bottoms of the conical vessels. The flat horizontal part of each of the platters, was now only 2 inches below the flat surface of the bottom of the conical vessel which was suspended over it. Both the platters still remained covered by their flat circular perforated covers of paper; but it should be remembered, that the circular hole in the centre of each of these covers was no less than 6 inches in diameter, and consequently, that a large portion of the flat part of the bottom of the platter was in full view (if I may use that expression) of the bottom of the vessel which was suspended over it.

The times of cooling, in this experiment, were as follows.

No. 5 cooled through the given interval in 40¾ minutes.

No. 6, which was over ice - - in 32½ minutes.

The results of these experiments show, (what indeed might have been expected, especially on a supposition that the heating and cooling of bodies is effected by means of radiations,) that although the cooling of the hot body suspended over a surface kept constantly cold by artificial means, was accelerated by being brought nearer to that cold surface, yet, in a case where the cold surface was less intensely cold, and where its tempera-

ture could be sensibly raised by the calorific rays from the hot body, the cooling of the hot body was retarded by a nearer approach of that cold surface.

From the results of these experiments we may safely conclude, that if the hot body, instead of being a conical vessel covered up on all sides except its flat bottom, had been a globe, and if this hot globe had been suspended in the centre of another larger thin hollow sphere, (this last being, at the beginning of the experiment, at the same temperature as the air and walls of the room,) the vicinity of the surface of this hollow globe, to the surface of the hot body, would have retarded the cooling of the hot body, in the same manner as the cooling of the conical vessel No. 5 was retarded in the foregoing experiments; and if, instead of inclosing the hot body in the centre of a single hollow sphere, of any given thickness, it were placed in the common centre of a number of much thinner concentric spheres, of different diameters, the time of cooling would be still more retarded.

By tracing the various operations which would take place in the cooling of the hot body, in this imaginary experiment, we shall become acquainted with the nature of those which actually take place, when the cooling of a hot body is prolonged by means of warm clothing.

From the results of several of the foregoing experiments we may conclude, that, supposing the thin concentric hollow spheres in which the hot body is confined to be made of metal, the cooling will be slower, if the surfaces of these spheres are polished, than if they are unpolished, or blackened; and hence we might very naturally be led to suspect, (what is probably true in fact,) that the *warmth* of any kind of substance used as

clothing, or its power of preventing our bodies from being cooled by the influence (frigorific radiations) of surrounding colder bodies, depends very much *on the polish of its surface.*

If, with the assistance of a microscope, we examine those substances which supply us with the warmest coverings, such for instance as furs, feathers, silk, &c. we shall find their surfaces not only smooth, but also very highly polished; we shall also find that, other circumstances being equal, those substances are the warmest which are the finest, or which are composed of the greatest number of fine polished detached threads or fibres.

The fine white shining fur of a Russian hare, is much warmer than coarse hair; and fine silk, as spun by the silk-worm, is warmer than the same silk twisted together into coarse threads; as I found by actual experiments, an account of which has already been laid before this Society, and published in the Philosophical Transactions.

I formerly considered the warmth of natural and artificial clothing, as depending *principally* on the obstacle it opposes to the motions of the cold air by which the hot body is surrounded; but, by a patient and careful examination of the subject, I have been convinced, that the efficacy of radiation is much greater than I had supposed it to be.

From the result of the experiment No. 31, we might be led to conclude, that a very small part only of the heat which a hot body appears to lose when it is cooled in air, is in fact communicated to that fluid; a much greater portion of it being communicated to other surrounding bodies at a distance; and, in one of my former experiments, a hot body was cooled, though it was placed in a TORRICELLIAN vacuum.

These researches appear to me to be the more interesting, as I have long been of opinion, that it must be by experiments of this kind, (showing in what manner the temperature of bodies are affected reciprocally, at different degrees of temperature, and at different distances,) that the hypothesis of radiation must be established, or proved to be unfounded.

When I speak of heat as being communicated to air *immediately* by a hot body which is cooled in it, I mean only, that it is not first communicated to other neighbouring bodies, and then given *by them* to the particles of air with which they happen to be in contact. In this last mentioned way, much of the heat, no doubt, which a hot body loses when cooled in air, is ultimately communicated to that fluid.

I am far from supposing that the particles of air which, coming into contact with a hot body, are heated in consequence of that near approximation, receive heat in any other *manner* than that in which other bodies, at a greater distance, receive it. If, in the one case, it be generated, or excited, by the agency of calorific rays, or undulations, caused by the hot body, it must, I am persuaded, be excited in the same manner in the other.

The reason why the particle of air which is in immediate contact with a hot body is heated, while other particles, near it, are not affected by the calorific rays from the hot body, which are continually passing by them, through the air, is, I conceive, because the particle heated is at *the surface of the fluid*, (air,) where these rays are either reflected, refracted, or absorbed; but, when a ray has once passed the surface of a transparent fluid, it proceeds straight forwards, without being farther affected by it, *and consequently without affecting it*, till it comes to the confines of the medium, or to the surface of some other body.

If this hypothesis of the communication, or rather *generation*, of heat, and of cold, by radiation, be true, it will enable us to explain, in a satisfactory manner, what has been called the *non-conducting power* of transparent fluids, with respect to heat; for, if heat be really communicated, or excited, in the manner above described, it is quite evident that *a perfectly transparent fluid* can receive heat only at its surface; and consequently, that heat cannot be propagated in such a fluid, by communication, from one particle of the fluid to another.

By a *transparent* fluid, I mean such an one as admits the calorific and frigorific rays, emitted by hot and by cold bodies, to pass freely through it, without obstructing their passage, or diminishing their intensities.

Whether any of the fluids with which we are acquainted be *perfectly* transparent in this sense of the word, or not, I will not pretend to say; but there is reason to think that pure water, and air, and most other fluids which are transparent to light, possess a high degree of transparency, in regard to calorific and frigorific rays; or that they give a very free passage to them, when they have once passed their surfaces.

An even or polished surface has been found to facilitate very much the reflection of the rays of light. May it not, in all cases, have an equal tendency to facilitate the reflection of calorific and frigorific rays?

In the experiments with the large cylindrical vessels, where they were exposed *naked* to cool in the air, their surfaces were polished, and they were a long time in cooling. But, when the surface of the vessel was blackened, or covered with other substances, the vessel was found to cool much more rapidly.

A large proportion of the frigorific rays from the surrounding

colder bodies were, in the former case, reflected at the polished surface of the metallic vessel; but, in the latter case, more of them were absorbed.

When a large drop of water rolls about, without being evaporated, upon the flat surface of a piece of red-hot iron, the surface of the drop is *polished;* and, the calorific rays being mostly reflected, the water is very little heated, notwithstanding the extreme intensity of the heat of the iron, and its nearness to the water.

If the iron be *less hot*, the water penetrates the pores of the oxide which covers the metal,—the drop ceases to have a polished surface,—acquires heat very rapidly,—and is soon evaporated.

If a drop of water be placed on the clean and polished surface of a metal not so easily oxidable as iron, it will retain its spherical form and polished surface, under a lower degree of temperature than on iron; and consequently will be less heated, and less rapidly evaporated by a moderate heat.

If a large drop of water be put carefully into a clean silver spoon, previously heated very hot, (that is to say, so hot as to give a loud hissing noise when touched with the wetted finger, but much below the heat of red-hot metal,) the drop will support, or rather *resist*, this heat for a considerable time; but, after the spoon has been suffered to cool down nearly to the temperature of boiling water, a drop of water put into it will be evaporated instantaneously.

It appears, from the results of these experiments, to be probable that, under high temperatures, air is attracted by metals so much more strongly than water, that even the weight of a drop of water is not sufficient to force away the stratum of air

which covers, and adheres to, the surface of a metal on which the drop reposes; but, at lower temperatures, this does not seem to be the case.

The following experiment, which I made several months ago, with a view to investigate the cause of the slow evaporation of drops of water placed on hot metals, will, I think, throw much light on this subject.

Exper. No. 34. Taking a clean polished silver spoon, I blackened the inside of it, by holding it over the flame of a wax candle; then, putting a large drop of water into it, I found, as I expected, that the drop took a spherical form, and rolled about in the spoon, without wetting its blackened surface.

I now held the spoon over the flame of a candle, and attempted to make the water boil; but I found it to be absolutely impossible. The handle of the spoon became so very hot, that I could not hold it in my hand without being burnt, though it was wrapped up in three or four thicknesses of linen; but still the drop of water did not appear to be at all affected by this intense heat. If the bowl of the spoon were touched with the finger, a hissing noise announced that it was extremely hot; but still, the water remained perfectly quiet in the spoon, without being evaporated.

Having in vain attempted to make this drop of water boil, and not being able to hold the spoon over the flame of the candle any longer, on account of the heat of its handle, I now poured the drop into the palm of my hand. I found it to be warm, but by no means scalding hot.

By holding the spoon, with a pair of tongs, over the flame of the candle for a longer time, I found that a drop of water in

the spoon gradually *changed its form*, became less, and was at length evaporated: from being spherical and lucid, it gradually took an oblong form, and its surface became obscure; and, when it was evaporated, it left a kind of skin behind it, which was evidently composed of the particles of black matter, which had by degrees attached themselves to its surface, and which probably had contributed not a little to its being at last heated, and evaporated.

The change in the form of the drop of water, and more especially the gradual loss of its lucid appearance, made me suspect that it had turned round during the experiment. If it really did so, its motion must either have been extremely rapid, or very slow; for, though I examined it with great attention, I could not perceive that it had any rotatory motion.

I will take the liberty to mention another little experiment, which I have often made, to amuse myself and others, though it may perhaps be thought too trifling to deserve the attention of the Royal Society.

Exper. No. 35. If a large drop of water be formed at the end of a small splinter of light wood, (deal, for instance,) and this drop be thrust quickly into the centre of the flame of a newly snuffed candle, which burns bright and clear, the drop of water will remain for a considerable time in the centre of the flame, and surrounded by it on every side, without being made to boil, or otherwise apparently affected by the heat; and, if it be taken out of the flame, and put upon the hand, it will not be found to be scalding hot.

If it be held for some time in the flame, it will be gradually diminished, by evaporation; but there is much reason to think,

that the heat which it acquires is not communicated to it by the flame, but by the wood to which it adheres, which is soon heated by the flame, and even set on fire.

I cannot refrain from just observing, that it appears to me to be extremely difficult to reconcile the results of any of the foregoing experiments, with the hypothesis of modern chemists respecting the *materiality of heat*.

Deeply sensible of the insufficiency of the powers of the human mind, to unfold the mysteries of nature, and discover the agents she employs, and their mode of action, in her secret and invisible operations; and being moreover fully aware of the danger of forming an attachment to a false theory, and of the folly of wasting time in idle speculations; I have ever, in my philosophical researches, been much more anxious to discover new facts, and to show how the discoveries of others may be made useful to mankind, than to invent plausible theories; which much oftener tend to misguide, than to lead us in the path of truth and science.

There are however situations, in which an experimental enquirer sometimes finds himself, where it is almost impossible for him to abstain from forming, or adopting, some general theory, for the purpose of explaining the phenomena which fall under his observation, and directing him in his future researches.

Finding myself in that situation at this time, I beg the attention, and above all the *indulgence*, of the Society, while I endeavour to explain the conjectures I have formed, respecting the nature of heat, and the mode of its communication.

Hot and *cold*, like *fast* and *slow*, are mere relative terms; and, as there is no relation, or proportion, between motion and a state of rest, so there can be no relation between any degree

of heat and absolute cold, or a total privation of heat: hence it is evident, that all attempts to determine the place of *absolute cold*, on the scale of a thermometer, must be nugatory.

It seems probable that *motion* is an essential quality of matter; and that rest is no where to be found in the universe.

We well know, that all those bodies which fall under the cognizance of our senses are in motion; and there are many appearances which seem to indicate, that the constituent particles of all bodies are also impressed with continual motions among themselves; and that it is these motions (which are capable of augmentation and diminution) that constitute the *heat* or temperature of sensible bodies.

The only effects of which we have any idea, resulting from the action of one body on another, are a change of velocity, or a change of direction, or both. We perceive, it is true, that certain bodies have a power of affecting certain other bodies *at a distance*; but this is no proof that the effects produced are essentially different from those which result from collision; for, if an elastic body be interposed between the two bodies, their actions on each other may be communicated through such intermediate elastic body, which, when the action is at an end, and the effects resulting from it on the two bodies have taken place, will be in the same state precisely in which it was before the action began.

If a bell, or any other solid body, *perfectly elastic*, placed in a perfectly elastic fluid, and surrounded by other perfectly elastic solid bodies, were struck, and made to vibrate, its vibrations would, by degrees, be communicated, by means of the undulations, or pulsations they would occasion in the elastic fluid medium, to the other surrounding solid and elastic bodies. If

these surrounding bodies should happen to be already vibrating, and with the same velocity as that with which the bell is made to vibrate by the blow, the undulations in the elastic fluid, occasioned by the bell, would neither increase nor diminish the velocity or frequency of the vibrations of the surrounding bodies; neither would the undulations caused by the vibrations of these bodies tend to accelerate, or to retard, the vibrations of the bell. But, if the vibrations of the bell were more frequent than those of the surrounding bodies, the undulations it would occasion in the elastic fluid, would tend to accelerate the vibrations of the surrounding bodies: on the other hand, the undulations occasioned by the slower vibrations of the surrounding bodies, would retard the vibrations of the bell; and the bell, and the surrounding bodies, would continue to affect each other, until, by the vibrations of the latter being gradually increased, and those of the former diminished, in consequence of their actions on each other, they would all be reduced to the same *tone*.

Supposing now, that heat be nothing more than the motions of the constituent particles of bodies among themselves, (an hypothesis of ancient date, and which always appeared to me to be very probable,) if for the bell we substitute a hot body, the cooling of it will be attended by a series of actions and reactions, exactly similar to those just described.

The rapid undulations occasioned in the surrounding ethereal fluid, by the swift vibrations of the hot body, will act as calorific rays on the neighbouring colder solid bodies; and the slower undulations, occasioned by the vibrations of those colder bodies, will act as frigorific rays on the hot body; and these reciprocal actions will continue, but with decreasing intensity, till the hot body, and those colder bodies which surround it, shall, in

consequence of these actions, have acquired the same temperature, or until their vibrations have become isochronous.

According to this hypothesis, *cold* can with no more propriety be considered as the absence *of heat*, than a low or grave sound can be considered as the absence of a higher or more acute note; and the admission of rays which generate cold, involves no absurdity, and creates no confusion of ideas.

On a superficial view of the subject, it may perhaps appear difficult to reconcile solidity, hardness, and elasticity, with those never-ceasing motions which we have supposed to exist among the constituent particles of all bodies; but a patient investigation of the matter will show, that the admission of that supposed fact, instead of rendering it more difficult to form distinct and satisfactory ideas of the causes on which those qualities of bodies depend, will rather facilitate those abstruse researches.

Judging from all the operations of nature, of the causes of which we are able to form any distinct ideas, we are certainly led to conclude, that the force of dead matter, (and perhaps of living matter also,) or its power of affecting, that is to say, of *moving*, other matter, or of *resisting its impulse*, depends on its motion.

If, therefore, solid (or fluid) bodies have any powers whatever, either of impulse or of resistance, it appears to me to be more reasonable to ascribe them to the living forces residing in them, —to the never-ceasing motions of their constituent particles,— than to suppose them to be derived from their want of power, and their total indifference to motion and to rest.

No reasonable objection against this hypothesis, (of the incessant motions of the constituent particles of all bodies,) founded on a supposition that there is not room sufficient for these

motions, can be advanced; for we have abundant reason to conclude, that if there be in fact any indivisible solid particles of matter, (which however is very problematical,) these particles must be so extremely small, compared to the spaces they occupy, that there must be ample room for all kinds of motions among them.

And, whatever the nature or directions of these internal motions may be, among the constituent particles of a solid body, as long as these constituent particles, in their motions, do not break loose from the systems to which they belong, (and to which they are attached by gravitation,) and run wild in the vast void by which each system is bounded, (which, as long as the known laws of nature exist, is no doubt impossible,) the form or external appearance of the solid cannot be sensibly changed by them.

But, if the motions of the constituent particles of any solid body be either increased or diminished, in consequence of the actions, or radiations, of other distant bodies, this event could not happen without producing some visible change in the solid body.

If the motions of its constituent particles were *diminished* by these radiations, it seems reasonable to conclude, that their elongations would become less, and consequently, that the volume of the body would be contracted; but, if the motions of these particles were increased, we might conclude, *a priori*, that the volume of the body would be expanded.

We have not sufficient data to enable us to form distinct ideas of the nature of the change which takes place when a solid body is melted; but, as fusion is occasioned by heat, that is to say, by

an augmentation (from without) of that action which occasions expansion, if expansion be occasioned by an increase of the motions of the constituent particles of the body, it is, no doubt, a certain additional increase of those motions, which causes the form of the body to be changed; and, from a solid, to become a fluid substance.

As long as the constituent particles of a solid body which are at the surface of that body, do not, in their motions, *pass by each other*, the body must necessarily retain its form or shape, however rapid those motions or vibrations may be; but, as soon as the motion of these particles is so augmented that they can no longer be restrained, or retained within these limits, the regular distribution of the particles, which they acquired in crystallization, is gradually destroyed; and the particles so detached from the solid mass, form new and independent systems, and become a liquid substance.

Whatever may be the figures of the orbits which the particles of a liquid describe, the mean distances of those particles from each other remain nearly the same as when they constituted a solid, as appears by the small change of specific gravity which takes place, when a solid is melted, and becomes a liquid; and, on a supposition that their motions are regulated by the same laws which regulate the solar system, it is evident that the additional motion they must necessarily acquire, in order to their taking the fluid form, cannot be lost, but must continue to reside in the liquid, and must again make its appearance, when the liquid changes its form, and becomes a solid.

It is well known that a certain quantity of *heat* is requisite to melt a solid; which quantity disappears, or remains *latent* in the

liquid produced in that process; and that the same quantity of heat reappears, when this liquid is congealed, and becomes a solid body.

But, before I proceed any farther in these abstruse speculations, I shall endeavour to investigate some of the consequences which would necessarily result from the radiations of hot and of cold bodies, supposing those radiations to exist, and their motions and actions to be regulated by certain assumed laws.

And first, it is evident that the intensity of the rays emitted by a luminous point, in a perfectly transparent medium, is every where as the squares of the distance from that point, inversely; for the intensity of those rays must be as their condensation; and their condensation being diminished, in proportion as the space they occupy is increased, if we suppose all the rays which proceed in all directions from any point, to set out at the same instant, and to move with the same velocity, in right lines, these simultaneous rays (or undulations) will, in their progress, form a sphere, which sphere will increase continually in size, as the rays advance; and, as all the rays must be found at the surface of this sphere, their intensity, or condensation, must necessarily be as the surface of the sphere, inversely, or as the squares of the distance, inversely, from the centre of the sphere, or, which is the same thing, from the luminous point from which these rays proceed; the surfaces of spheres being to each other as the squares of their radii.

Supposing now, (what indeed appears to be incontrovertible,) that the intensity of the rays which hot and cold bodies emit, in a medium perfectly transparent, follows the same law, we can determine what effects must be produced, by the largeness, or

smallness, of the confined space (of a room, for instance) in which a hot body is placed, to cool.

To simplify this investigation, we will suppose this confined space to be a hollow sphere of ice, 9 feet in diameter, at the temperature of freezing water; and the hot body to be a solid sphere of metal, 2 inches in diameter, at the temperature of boiling water, placed in the centre of it; and we will suppose farther, that this hollow sphere is void of air, and that the cooling of the hot body is effected solely by the frigorific rays from the ice.

The question to be determined is, in what manner the cooling of the hot body would be affected, by increasing the diameter of this hollow sphere of ice?

Let us suppose its diameter to be increased to 18 feet. Its internal surface will then be to the surface of a sphere 9 feet in diameter, as the square of 18 to the square of 9, that is to say, as 324 to 81, or as 4 to 1. And, as the quantity of frigorific rays emitted are, *cæteris paribus*, as the surface from which they proceed, the quantity of rays emitted by the internal surface of the larger sphere, will be to the quantity emitted by the internal surface of the smaller, as 4 to 1.

But the intensities of these rays, at the common centre of these spheres, (where the hot body is placed,) being as the squares of the distances from the radiating points, inversely, the intensity of the rays from the internal surface of the smaller sphere, must be to the intensity of the rays from the internal surface of the larger sphere, as 4 to 1, at the common centre of those spheres.

Now, as the time of the cooling of the hot body will depend

on the *quantity* of frigorific rays which arrive at its surface, and on the *intensity* of their action; and, as the intensity of the rays from the internal surface of the sphere, at its centre, is diminished in the same proportion as the surface of the sphere is augmented when its diameter is increased; it follows, that a hot body placed in the centre of a hollow sphere, at any given constant temperature below that of the hot body, will be cooled in the same time, or with the same celerity, whatever may be the size of the sphere.

If this conclusion be well founded, (and I see no reason to suspect that it is not so,) it will follow, from the principles assumed, that the hot body will be cooled in the same time, in whatever part of the hollow sphere it be situated. And, as the cooling of the body is not affected, that is to say, accelerated, or retarded, either by the greater or smaller size of the inclosed space in which it is confined, or by its situation in that confined space, so it cannot be in any manner affected, either by the form of that hollow space, or by the presence of a greater or less number of other solid bodies; provided always, that all these surrounding bodies be at the same constant temperature.

If, however, any of these surrounding bodies, the temperature of which is liable to be sensibly changed during the experiment, by the calorific rays emitted by the hot body, be placed *very near* that body, the cooling of that hot body will be retarded; the rays from this neighbouring body, *so heated*, being less frigorific than those from other bodies at a greater distance, which it intercepts.

The results of all my experiments on the cooling of bodies, tended uniformly to confirm the above conclusions.

Admitting that the cooling of a hot body is effected solely by

the rays which proceed from colder bodies, and that these rays, like those of light, are reflected, refracted, and concentrated, according to certain known laws, by the polished surfaces of mirrors and lenses, it might perhaps be imagined, that the cooling of a hot body might be accelerated, or retarded, by giving it some peculiar form; or by placing near it, and in certain positions with respect to it, two or more highly polished reflecting mirrors.

As these conjectures, if well founded, might lead to experiments from the results of which the truth or falsehood of the hypothesis in question might be demonstrated, it is of much importance that this matter should be thoroughly investigated. I shall therefore beg the indulgence of the Society, while I endeavour to examine it with that careful attention which it appears to me to deserve.

When different solid substances, heated to the same degree of temperature, are exposed in the air to cool, those among them which appear to the touch to be the hottest, are not those which cool the fastest, or which send off calorific rays, through the air, in the greatest abundance.

As polished metals reflect a great part of the rays from other bodies which arrive at their surfaces, and as they are neither heated nor cooled by the rays so reflected, their temperatures are slowly changed by the actions of the surrounding bodies at a different temperature.

When a hot polished metallic body is exposed in the air to cool, surrounded by other bodies at the same temperature as that of the cold air, as most of the rays from the surrounding bodies are reflected at the polished surface of the hot body, it is evident that two sorts of rays must proceed from the surface of

that body, namely, those calorific rays which that hot body emits, and those other rays (which with regard to the surrounding bodies are, neither calorific nor frigorific) which it reflects.

On a cursory view of the subject, one might be led to imagine, that, as the rays which proceed from the hot metallic body are of two kinds, the energy of the calorific rays, which properly belong to the hot body, might be diminished by those other reflected rays by which they are accompanied, and with which they may be said to be mixed; but, a more careful examination of the matter will show that this cannot be the case; that is to say, as long as all the surrounding bodies continue to be at the same temperature. If the temperature of the surrounding bodies be different, such of them will be affected, by the reflected rays, as happen to be of a temperature different from that from which the ray originated; but still, the effects produced by the rays emitted by the hot body, will be the same, or their power of effecting changes in the temperatures of other (hotter or colder) bodies, will remain undiminished, and unchanged.

The reason why their effects are not more powerful than they are found to be, is not because they are mixed with other reflected rays, but because they are few; the greater part of the rays which the hot body actually emits being reflected, and turned back upon itself, by the reflecting surface by which it is immediately surrounded.

That the reflecting surface at which the rays of light are turned back and reflected, which impinge against the polished surface of any solid or fluid body, is actually situated *without the body*, and even at some distance from it, has been proved by the most decisive experiments; and there are so many striking

analogies between the rays of light and those invisible rays which all bodies, at all temperatures, appear to emit, that we can hardly doubt of their motions being regulated by the same laws.

Perhaps there may be no other difference between them, than exists between those vibrations in the air which are audible, and those which make no sensible impression on our organs of hearing.

If the ear were so constructed that we could hear all the motions which take place in the air, we should, no doubt, be stunned with the noise; and, if our eyes were so constructed as to see all the rays which are emitted continually, by day and by night, by the bodies which surround us, we should be dazzled and confounded by that insupportable flood of light, poured in upon us on every side.

Taking it for granted that these invisible radiations exist, we will endeavour to trace the effects which must necessarily be produced by them, in order to see if these investigations will not lead us to a discovery of the causes of some appearances which have hitherto been enveloped in much obscurity.

Suppose two concave reflecting mirrors, of highly polished metal, each 18 inches in diameter, and 18 inches focal distance, to be placed opposite to each other, at the distance of 10 feet, in a large quiet room, in which the air, and the walls of the room, remain constantly at the same temperature, (that of freezing water, for instance,) without any variation.

If we suppose the floor, ceiling, walls of the room, and doors and windows, to be lined with a covering of ice, at the temperature of freezing water, we can then, without any difficulty, conceive that the temperature of the room may remain the same,

notwithstanding the presence of hotter bodies, which are brought into it for the purpose of making experiments.

Let us now suppose one of the mirrors to be at the temperature of freezing, and the other at that of boiling water; and let us see what effects they would produce, on each other, by their radiations.

And first, with respect to the hot mirror, it is evident that it will be cooled, not only by the frigorific rays which proceed from the cold metal of which the opposite mirror is constructed, but also by such of the frigorific rays from the sides of the room as, impinging against the polished reflecting surface of the cold mirror, and being reflected by that surface, happen to fall on the surface of the hot mirror, without being reflected by it.

But, as the quantity of rays which the cold mirror *reflects* is greater, in proportion as the reflecting surface is more perfect, while the quantity of rays emitted by this cold mirror is less, in proportion as its reflecting surface is more perfect, it is extremely probable that the *total* quantity of frigorific rays (emitted and reflected) which, coming from the surface of the cold mirror, impinge against the surface of the hot mirror, will be the same, whatever may be the degree of polish, or reflecting power, of the cold mirror. And, if this be the case, we may conclude, that the presence of this mirror will have no effect whatever on the hot mirror; or, that it will no more expedite its cooling than any other body, of any other form, would do, at the same distance, and occupying the same space.

It might perhaps be imagined, that the *form* of the cold mirror might concentrate the rays it emits and reflects, and, by such concentration, produce a greater effect on the opposite mirror than if its surface were flat, or of any other form; but a more

attentive examination of the matter will show, that no such concentration actually takes place: for, with regard to those rays which are *emitted* by this cold body, as they proceed from each point of its surface *in all directions*, it is perfectly evident that these are not concentrated; and, with respect to those which are *reflected*, it is equally certain that they are not concentrated; because, in order to their being concentrated, they must arrive at the surface of the mirror in parallel lines, and in the direction of the axis of the mirror, which, under the given circumstances, is evidently impossible.

Hence we see, that the presence of the cold mirror will not tend, in the smallest degree, either to accelerate, or to retard, the cooling of the hot mirror; that is to say, provided its temperature be not raised by the calorific rays from the hot mirror.

If its temperature be raised by those rays, it will tend to retard the cooling of the hot mirror; but, even in this case, it will not retard it more than any other polished metallic body would do, of any other form, having the same area, or quantity of surface opposed to the hot mirror, and being placed at the same distance from it.

By a similar train of reasoning, it may be shown, that the *form* of the hot body (that of a concave mirror) will contribute nothing to the effect it will produce on the cold mirror, in heating it, by the calorific rays it emits; and that it will itself be cooled neither faster nor slower, on account of its peculiar form.

Let us now suppose both mirrors to be at the temperature, precisely, of the room, (that of freezing water;) and, that a bullet, or other small body of a spherical form, at the temperature of boiling water, be placed in the focus of one of the mirrors; which mirror we shall call A.

As the rays emitted by this hot body are sent off in right lines, in all directions, in the same manner as light is emitted by luminous bodies, all those rays which fall on the concave polished surface of the mirror A, will be reflected (as is well known) in lines nearly parallel to the axis of the mirror; they will consequently fall on the concave polished surface of the opposite mirror B; and, being there again reflected, they will be *concentrated* at the focus of the second mirror.

If now a sensible thermometer, at the temperature of the room, be placed in this focus, it will immediately begin to rise, in consequence of the heat generated in it by the action of these calorific rays, so accumulated in that place.

If, instead of being placed in the focus of this second mirror, the thermometer be placed at a very small distance from that focus, on one side of it, the instrument, however sensible it may be, will not be apparently affected by the rays from the hot body.

This experiment, which is of ancient date, has often been made, and always with the same results.

Let us now suppose the hot body to be removed from the focus of the mirror A; and that a colder body be substituted in place of it. And, in the first place, we will suppose the temperature of this colder body to be that of freezing water, or just equal to that which reigns in the room.

As the rays which bodies at the same temperature send off from one to the other, have no tendency to increase, or to diminish, the temperature of those bodies, the concentration of rays in the focus of the mirror B, proceeding from the ice-cold body placed in the focus of the mirror A, can have no effect on a

thermometer, at the same temperature, which is exposed to their action.

If heat be a vibratory motion of the constituent particles of bodies, and if the rays which sensible bodies send off in all directions be undulations in an ethereal elastic fluid by which they are surrounded, occasioned by those motions; as the pulsations in this fluid must be isochronous with the vibrations by which they are occasioned, these pulsations or undulations can neither accelerate nor retard the vibrations of other bodies at the surfaces of which they arrive; provided the vibrations of the constituent particles of such bodies are, at that time, isochronous with the vibrations of the constituent particles of the body from which these undulations proceed. But, to return to our experiment.

Suppose now that, instead of this ice-cold body, another much colder, at the temperature of freezing mercury, for instance, be placed in the focus of the mirror A, and that a thermometer at the temperature of freezing water be placed in the focus of the mirror B; what might be expected to be the result of this experiment?—That the thermometer would fall, in consequence of its being cooled by the accumulation of frigorific rays proceeding from this very cold body.

Now this is what actually happened, in the celebrated experiment of my ingenious friend Professor PICTET, of Geneva.

Several attempts have been made to explain the result of that experiment, on the supposition that caloric has a real or material existence, and that radiant heat is that substance, emitted and sent off in right lines, in all directions, from the surfaces of hot bodies. But none of these explanations appear to me to be

satisfactory. One of the most plausible of them, is that which is founded on a supposition that caloric is emitted continually, under the form of radiant heat, by all bodies, at all temperatures; but in greater abundance by hot bodies than by such as are colder; and that a body, at the same time that it sends off radiant caloric in all directions, to the bodies by which it is surrounded, receives it in return, in greater or less quantities, from all those bodies;—that, in all cases where a body, in any given time, receives more radiant caloric than it gives off, an accumulation of caloric in the body takes place, in consequence of which accumulation it becomes hotter;—but, when it gives off more caloric in any given time than it receives, its quantity of caloric is gradually diminished, and it becomes colder;—and, that a constant temperature results, from the quantities of caloric emitted and received continually being equal. But, besides the difficulty of explaining how, or by what mechanism, it can be possible for the same body to receive and retain, and reject and drive away, the same kind of substance, at one and the same time, (an operation not only incomprehensible, but apparently impossible, and to which there is nothing to be found analogous, to render it probable,) many other reasons might be brought to show, that this hypothesis, of the supposed continual interchanges of caloric between neighbouring bodies, is very improbable; and, among the rest, there is one which appears to me to be quite conclusive.

As the point in dispute seems to be of great importance to the science of heat, I shall endeavour to examine it with all possible attention; and, in order to put the hypothesis in question to the test, we will see if it will accord with the results of

some of the foregoing experiments; which, in order to their being more easily comprehended and examined, I shall elucidate by figures.

Let the two opposite ends of the cylinders A and B (Plate V. Fig. 4) represent the two vertical metallic disks, of equal dimensions, which were presented, at the same time, to the ball of the thermoscope C, in the experiment No. 23.

In that experiment, the disk A being at the temperature of 32° F. (that of freezing water,) and the disk B at 112° F. while the ball of the thermoscope C, and all other surrounding bodies, were at 72°, it was found, that the temperature of the thermoscope was not changed by the simultaneous actions of these two bodies, the one hot, and the other cold.

In order to account for this result, on the hypothesis before mentioned, we must begin by supposing that the ball of the thermoscope gives off radiant caloric continually, in all directions, and receives it, in return, from the surfaces of all the bodies by which it is surrounded.

With regard to all these surrounding bodies, (excepting the disks A and B,) as they are at the same temperature as the ball of the thermoscope, (that of 72°,) they will give continually to that instrument, just as much radiant caloric as they receive from it; and no change of temperature will result from these equal interchanges.

But, in respect to the disk A, as that is colder than the ball of the thermoscope, it returns to it a smaller quantity of radiant caloric than it receives from it; consequently, the thermoscope receives continually less than it gives: it would of course be gradually exhausted of caloric, and become colder, were it not

for the compensation it receives for this loss, from the disk B. This disk, being hotter than the thermoscope, gives to it continually, more radiant caloric than it receives from it; and, were it not for the simultaneous loss of caloric which the instrument sustains, in its interchanges with the cold disk A, its quantity of caloric would be augmented, and it would become hotter.

Now, as the temperature of the ball of the thermoscope is an arithmetical mean between that of the disk A and that of the disk B, it is reasonable to suppose, that the thermoscope receives just as much more caloric from B than it gives to it, as it gives to A more than it receives from it; and, if that be the case in fact, it is evident, that the simultaneous actions of the two disks on the ball of the thermoscope (or the traffic which they carry on with it in caloric) can neither tend to increase, nor to diminish, the original stock of that substance belonging to that instrument; consequently, the instrument will neither be heated, nor cooled, by these interchanges, but will continue invariably at the same constant temperature.

This explanation is plausible; but, before the hypothesis on which it is founded can be admitted, we must see if it will agree with the results of other experiments; for the greatest care ought always to be used in the admission of hypotheses in physical researches; and, in no case can it be more indispensably necessary, than where an hypothesis has evidently been contrived for the sole purpose of explaining a single experiment, or elucidating a new fact.

When the surface of the metallic disk B was blackened, by holding it over the flame of a candle, the intensity of its radiation,

at the given temperature, (that of 119°,) was found to be very considerably increased; and when (being so blackened) it was again presented to the ball of the thermoscope, at the same distance as in the last-mentioned experiment, and the cold disk A (at the temperature of 32°) was placed opposite to it, at an equal distance, as represented in Fig. 5, the thermoscope, instead of continuing to retain its original temperature, (that of 72°,) was now gradually heated.

There is nothing, it is true, in that event, which appears difficult to explain on the assumed principles; for, if the quantity of radiant caloric emitted by the disk B, be increased by blackening its surface, the quantity received from it by the ball of the thermoscope must be increased also; and that additional quantity must of course tend to raise the temperature of the instrument. But here is an experiment which cannot be explained on those principles.

The surface of the cold disk A having been blackened, as well as that of the hot disk B, when both disks (blackened) were again presented, at equal distances, to the ball of the thermoscope, as represented in Fig. 6, it was found, that the original temperature of the thermoscope remained unchanged.

The result of this most interesting experiment proves, that the ball of the thermoscope was just as much cooled by the influence of the cold blackened disk, as it was heated by the hot blackened disk.

Now, as it was found by experiment, that the intensity of the radiation of the disk B was *increased* by the blackening of the surface of that disk, we must conclude, that the intensity of the radiation of the disk A was likewise *increased* by the use

of the same means: but, if those radiations be *caloric*, emitted by those bodies, (which the hypothesis in question supposes,) how did it happen, that the ball of the thermoscope, instead of being *more heated* by the additional quantity of caloric which it received in consequence of the blackening of the disk A, was actually *more cooled*?

It may perhaps be said, by the advocates for the hypothesis in question, that the blackening of the surface of the disk A, caused a greater quantity of caloric to be sent off to it by the ball of the thermoscope. Without insisting on an explanation of the mode of action of the cause which is supposed to produce this effect, (which I might certainly do, as the supposition is perfectly gratuitous,) I will content myself with just observing, that as the surface of the opposite disk *was also blackened*, this supposed augmentation of the quantity of caloric emitted by the ball of the thermoscope, *occasioned by the blackening of the surfaces of the bodies presented to it*, can be of no use in explaining the phenomena in question.

The results of the two last mentioned experiments appear to me to be very important; and I do not see how they can be reconciled with the opinions of modern chemists, respecting the nature of heat.

In order to simplify our speculations on this abstruse subject, we have hitherto supposed, that *difference of temperature* depends solely on the difference *of the times* of the vibrations of the component particles of bodies. It is possible, however, and even probable, that it depends principally on the *velocities* of those particles: for it is easy to perceive, that the more rapid the motions of those particles are, the greater their elongations must

be, in their vibrations; and the more, of course, will the volume of the body they compose be expanded.

It is well known, that the pulsations occasioned in an elastic fluid, by the vibrations of an elastic solid body, proceed from that body in all directions; and that these pulsations are every where (that is to say, at all distances from the body) isochronous with the vibrations of the solid body; it is known also, that the mean velocity of any individual particle of the fluid is less, in proportion as the distance of the particle is greater from the centre from which these pulsations proceed.

In the case of the pulsations occasioned in the air by the vibrations of sonorous bodies, those pulsations are every where isochronous with the vibrations of the sonorous body; and the time, or *frequency* of those pulsations, determines the *note*; but it is the *velocity* of the particles of the air, or the breadth of the wave, on which the *force* or *strength* of the sound depends; and this velocity becoming less, as the distance from the sonorous body increases, the sound is weakened in the same proportion.

There are several circumstances which might lead us to suspect, that *colour* depends on the *frequency* of those pulsations which have been supposed to constitute light; and that the *heat* produced by them is in proportion to their *force*.

If this supposition should be well founded, a knowledge of that important fact might perhaps enable us to explain several very interesting phenomena;—the combustion of inflammable bodies, for instance; and the great intensity of the heat which is produced by the *concentration* of calorific rays.

There are several well known experiments with burning glasses, which show that the intensity of the heat generated by

the concentration of the solar rays, is not simply as the *condensation* of those rays, but in a higher proportion; and, that it depends much on their *direction*, being greater, as the angle is greater at which they meet at the focus of the lens.

That fact is certainly very remarkable. It has often been the subject of my meditations; and it has contributed not a little to the opinion I have been induced to adopt, respecting the nature of light and of heat. I never could reconcile it with the supposition that heat is caused by the *accumulation* of any thing *emitted* by the sun; or by any other body which sends off calorific radiations.

Reserving for a future communication, an account of the sequel of my enquiries respecting the subject which I have undertaken to investigate, I shall conclude this long Paper with some observations, concerning the *practical uses* that may be derived from a knowledge of the facts which have been established by the results of the foregoing experiments.

In all cases where it is designed to *preserve the heat* of any substance which is confined in a metallic vessel, it will greatly contribute to that end, if the external surface of the vessel be very clean and bright: but, if the object be to *cool* any thing quickly, in a metallic vessel, the external surface of the vessel should be painted, or covered with some of those substances which have been found to emit calorific rays in great abundance.

Polished tea-urns may be kept boiling hot with a much less expence of spirit of wine (burnt in a lamp under them) than such as are varnished; and the cleaner and brighter the dishes, and covers for dishes, are made, which are used for bringing

victuals on the table, and for keeping it hot, the more effectually will they answer that purpose.

Saucepans, and other kitchen utensils, which are very clean and bright on the outside, may be kept hot with a smaller fire than such as are black and dirty; but the bottom of a saucepan, or boiler, should be blackened, in order that its contents may be made to boil quickly, and with a small expence of fuel.

When kitchen utensils are used over a fire of sea-coal, or of wood, there will be no necessity for blackening their bottoms, for they will soon be made black by the smoke; but, when they are used over a clear fire made with charcoal, it will be adviseable to blacken them; which may be done in a few moments, by holding them over a wood or coal fire, or over the flame of a lamp, or candle.

Proposals have often been made for constructing the broad and shallow vessels (flats) in which brewers cool their wort, of metal; on a supposition that the process of cooling would go on faster in a metallic vessel than in a wooden vessel; but this would not be found to be the case in fact, a metallic surface being ill calculated for expediting the emission of calorific rays.

The great thickness of the timber of which brewers flats are commonly made, is a circumstance very favourable to a speedy cooling of the wort; for, when the flats are empty, this mass of wet wood is much cooled, not only by the cold air which passes over it, but also, and more especially, by evaporation; and, when the flat is again filled with hot wort, a great part of the heat of that liquid is absorbed by the cold wood.

In all cases where metallic tubes filled with steam are used

for warming rooms, or for heating drying-rooms, the external surface of those tubes should be painted, or covered with some substance which facilitates the emission of calorific rays. A covering of thin paper will answer that purpose very well, especially if it be black, and if it be closely and firmly attached to the surface of the metal with glue.

Tubes which are designed for *conveying* hot steam from one place to another, should either be well covered up with *warm* covering, or should be kept clean and bright. It would, I am persuaded, be worth while, in many cases, to gild them, or at least to cover them with what is called gilt paper, or with tin foil, or some other metallic substance which does not easily tarnish in the air.

The cylinders, and principal steam-tubes of steam-engines, might be covered, first with some warm clothing, and then with thin sheet brass, kept clean and bright. The expence of this covering would, I am confident, be amply repaid, by the saving of heat and fuel which would result from it.

If garden walls painted black acquire heat faster, when exposed to the sun's direct rays, than when they are not so painted, they will likewise cool faster, during the night; and gardeners must be best able to determine whether these rapid changes of temperature are, or are not, favourable to fruit trees.

Black clothes are well known to be very warm in the sun; but they are far from being so in the shade, and especially in cold weather. No coloured clothing is so cold as black, when the temperature of the air is below that of the surface of the skin, and when the body is not exposed to the action of calorific rays from other substances.

It has been shown, that the warmth of clothing depends much on the *polish* of the surface of the substance of which it is made; and hence we may conclude that, in choosing the colour of our winter garments, those dyes should be avoided which tend most to destroy that polish: and, as a white surface reflects more light than an equal surface, equally polished, of any other colour, there is much reason to think that white garments are warmer than any other, in cold weather. They are universally considered as the coolest that can be worn, in very hot weather, and especially when a person is exposed to the direct rays of the sun; and, if they are well calculated to reflect calorific rays in summer, they must be equally well calculated to reflect those frigorific rays by which we are cooled and annoyed in winter.

I have found, by direct and decisive experiments, (of which an account will hereafter be given to this Society,) that garments of fur are much warmer, in cold weather, when worn with the fur or hair outwards, than when it is turned inwards. Is not this a proof that we are kept warm by our clothing, not so much by confining our heat, as by keeping off those frigorific rays which tend to cool us?

The fine fur of beasts, being a highly polished substance, is well calculated to reflect those rays which fall on it; and, if the body were kept warm by the rays which proceed from it being reflected back upon it, there is reason to think, that a fur garment would be warmest when worn with the hair inwards; but, if it be by reflecting and turning away the frigorific rays from external (colder) bodies, that we are kept warm by our clothes in cold weather, we might naturally expect, that

a pellisse would be warmest when worn with the hair outwards, as I have found it to be in fact.

The point here in question is by no means a matter of small importance; for, until the principles of the warmth of clothing be understood, we shall not be able to take our measures with certainty, and with the least possible trouble and expence, for defending ourselves against the inclemencies of the seasons, and making ourselves comfortable in all climates.

The fur of several delicate animals becomes white in winter, in cold countries; and that of the bears which inhabit the polar regions, is white in all seasons. These last are exposed alternately, in the open air, to the most intense cold, and to the continual action of the sun's direct rays during several months. If it should be true that heat, and cold, are excited in the manner above described, and that white is the colour most favourable to the reflection of calorific and frigorific rays, it must be acknowledged, even by the most determined sceptic, that these animals have been exceedingly fortunate, in obtaining clothing so well adapted to their local circumstances.

The excessive cold which is known to reign, in all seasons, on the tops of very high mountains, and in the higher regions of the atmosphere, and the frosts at night, which so frequently take place on the surface of the plains below, in very clear and still weather, in spring and autumn, seem to indicate, that frigorific rays arrive continually at the surface of the earth, from every part of the heavens.

May it not be by the action of these rays that our planet is cooled continually, and enabled to preserve the same mean temperature for ages, notwithstanding the immense quantities

of heat that are generated at its surface, by the continual action of the solar rays?

If this conjecture should be well founded, we should be led to conclude, that the inhabitants of certain hot countries, who sleep at night on the tops of their houses, in order to be more cool and comfortable, do wisely, in choosing that situation to pass their hours of rest.

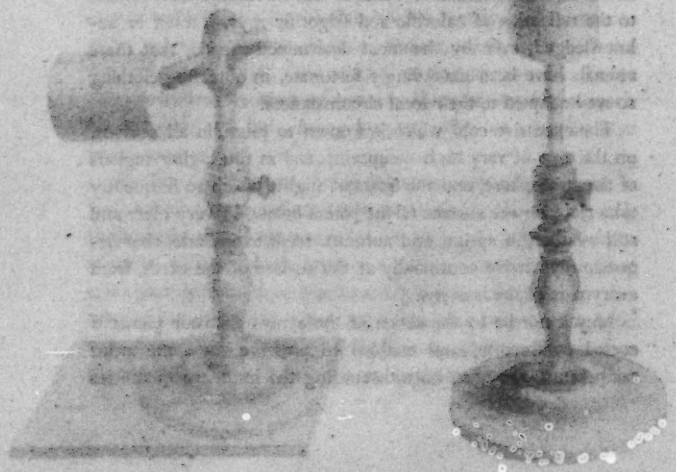


Fig. 1.

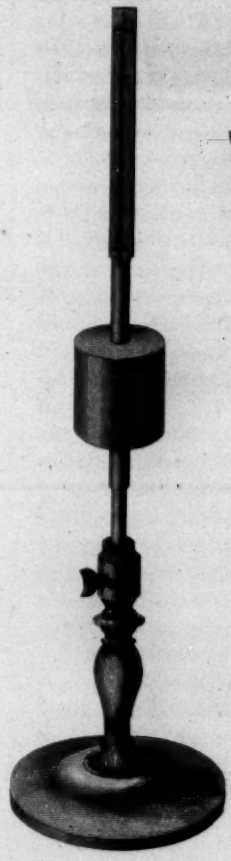


Fig. 2.

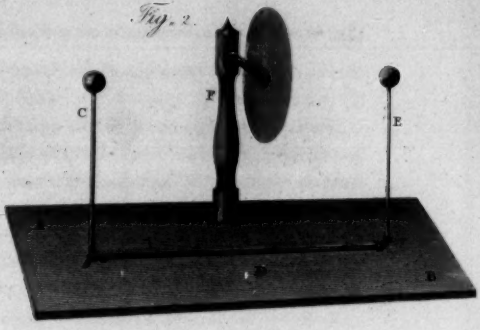
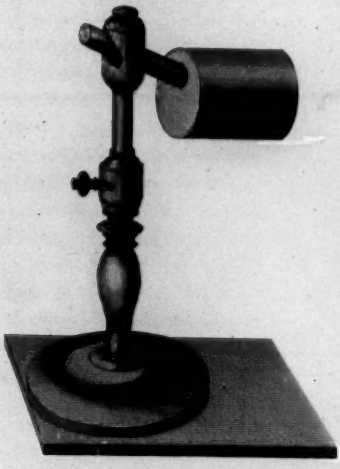


Fig. 3.



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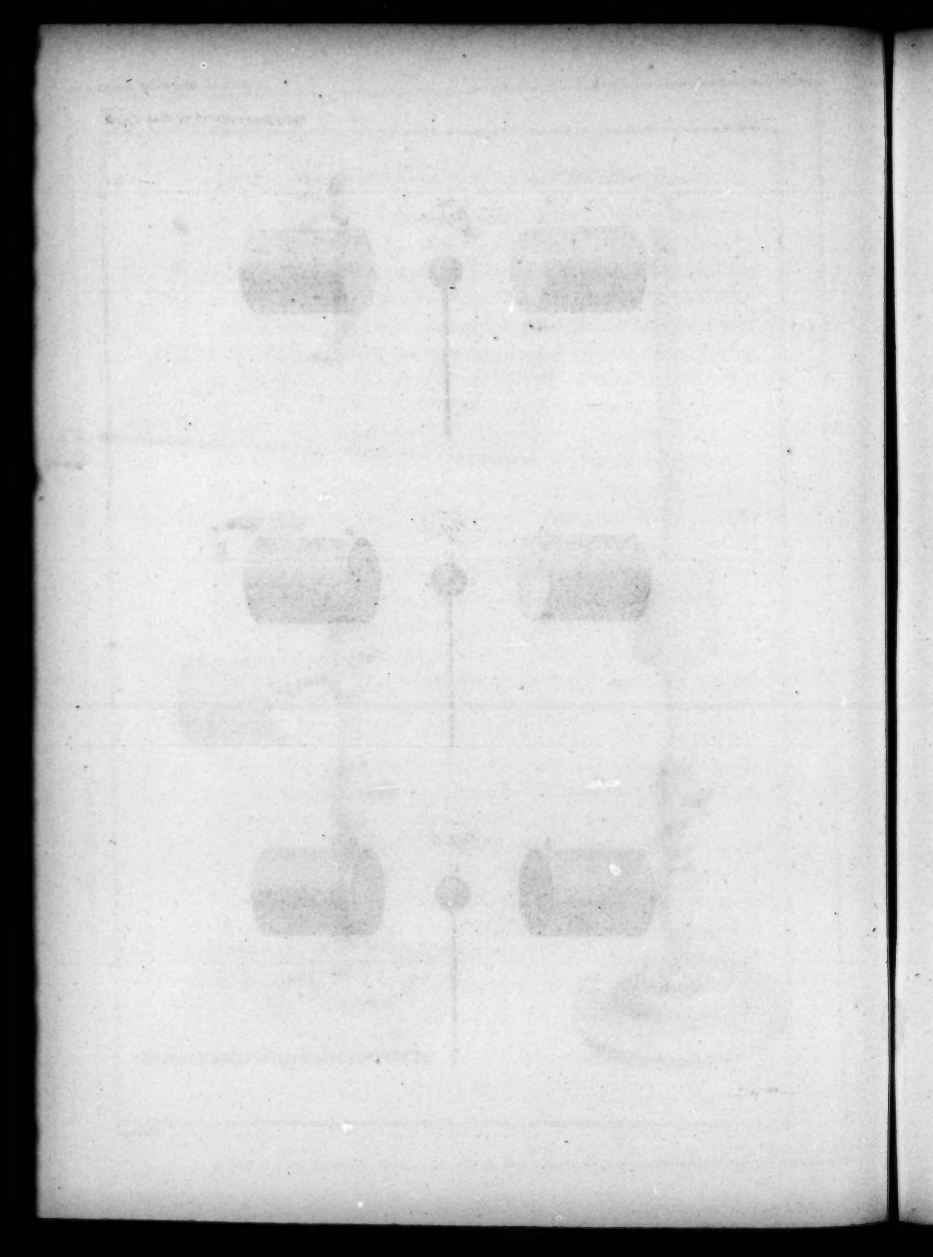


Fig. 4.

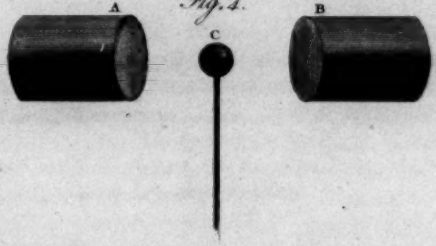


Fig. 5.

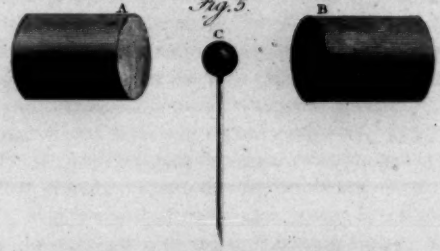
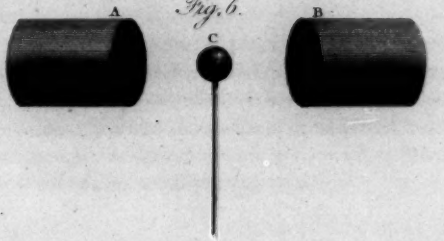


Fig. 6.



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VIII. *Experiments and Observations on the Motion of the Sap in Trees. In a Letter from* Thomas Andrew Knight, *Esq. to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read February 16, 1804.

MY DEAR SIR,

In the Observations on the Descent of the Sap in Trees, which I last year took the liberty to request you to lay before the Royal Society, I offered a conjecture, that the vessels of the bark, which pass from the leaves to the extremities of the roots, were, in their organization, better calculated to carry the fluids they contain towards the roots than in the opposite direction. I had not, however, at that time, any experiment directly to support this supposition; but I thought the forms generally assumed by trees in their growth, evinced the compound and contending actions of gravitation, and of an intrinsic power in the vessels of the bark, to give motion to the fluid passing through them. In the account of the experiments which I have now the honour to address to you, I trust I shall be able to adduce some interesting facts, in support of that inference.

Having selected, in the spring of 1802, four strong shoots of the vine, growing along the horizontal trellis of my vinery, I depressed a part of each shoot, whilst it was soft and succulent, about three inches deep, into the mould of a pot placed beneath it for that purpose; but without making any wound, or incision, in the young shoots thus employed as layers.

In this position they remained during the succeeding summer; and, in the autumn, had nearly filled the pots, which were ten inches in diameter, with their roots. As soon as the leaves had fallen, the layers were disengaged from the parent stocks; and about five inches of wood, containing one bud, were left, both at the proper and the inverted end of each layer. Every bud was also, by previous management, made to stand at an equal distance from the mould in the pots, and with an equal elevation, of about thirty-six degrees. About one inch of wood was likewise left at each end of every layer, beyond the buds.

In the succeeding spring, the buds vegetated strongly, both at the proper and at the inverted ends of the layers, as the experiments of HALES and DU HAMEL had given me reason to expect; and, in one instance, the bud at the inverted end of the layer grew with greater vigour than that at its proper end: but the growth of these buds was not the object which I had in view.

I have already stated, that nearly an inch of wood was left at each end of every layer, beyond the bud; and, to this wood, at the inverted ends of the layers, my attention was chiefly directed: for, if the vessels of the bark possessed the powers I attributed to them, I concluded that the sap would be impelled to the inverted ends of the layers, and be there employed in the production of new wood, and roots; and, in this, my expectations were not disappointed. At the proper end of the layers, the wood immediately beyond the buds became dry and lifeless, early in the succeeding summer; the stems also, between the buds and the mould in the pots, increased in size as usual; and nothing peculiar occurred. But, at the inverted end, appearances were extremely different: new wood here accumulated rapidly beyond

the buds, and numerous roots, of considerable length, were emitted, whilst no sensible growth took place between the base of the young shoots and the mould in the pots.

It having been proved by Du Hamel, that inverted parts of trees readily emit roots, I expected to derive further information from cuttings of this kind: I therefore planted, in the autumn of 1802, forty cuttings of the gooseberry-tree, and an equal number of the common currant-tree; one half of each being inverted. Of the former, not one of the inverted cuttings succeeded; whereas few of the latter failed; and in these I had an opportunity of observing the same accumulation of wood above the bases of the annual shoots, and the same mode of growth, in every respect, as in the inverted vines; except that no roots were emitted at their upper ends. The same thing occurred, without any variation, in inverted grafts of the apple-tree.

If it be admitted, according to the theory I have on a former occasion laid before you, that the sap descends from the leaves through the vessels of the bark, and that such vessels are, in their organization, better calculated to carry their contents towards the original roots than in the opposite direction, it will be extremely easy to explain the cause of the accumulation of wood, and the emission of roots, above, instead of below, the base of the annual shoots. The vessels of the bark (the *vaisseaux propres* of Du Hamel) commencing in the leaves, were formerly traced by M. Mariotte, and subsequently by myself, (being ignorant of his discovery,) to the extremities of the roots; and, when a cutting, or tree, is planted in its natural position, the sap passes downwards through these, to afford matter for new roots, and to increase the bulk of those already formed, having given proper nutriment to the branches and trunk in its descent.

But, in the inverted cutting, or tree, these vessels become inverted; and, if their organization be such as I have supposed it, a considerable part of that fluid, which naturally descends, will be carried upwards, and occasion the production of new wood, above, instead of below, the junction of the annual shoot with the older wood, as in the experiments I have described. The force of gravitation will, however, still be felt; and, by its agency, sufficient matter to form new roots may be conveyed to those parts of the inverted cutting, or tree, which are beneath the soil. Besides, if we suppose a variation to exist in the powers or organization of the vessels which carry the sap towards the root, we may also attribute, in a great measure, to this cause, the different forms which different species or varieties of trees assume; for, if the fluid in these vessels be impelled with much force towards the roots, little matter will probably be deposited in the branches, which, in consequence, will be slender and feeble, as in the vine; and there is not any tree that has been the subject of my experiments, in which new wood accumulated so rapidly at the upper end of inverted plants. To an excess of this power, in the vessels of the bark, we may also ascribe the peculiar growth of what are called weeping trees; for, by this power, the effects of gravitation will be, in a great degree, suspended; and the pendant branch will continue healthy and vigorous, by retaining its due circulation. The perpendicular branch will, however, still possess some advantages; for, in this, gravitation will act on the fluid descending from the leaves; and these will of course absorb from the atmosphere with increased activity. A greater quantity of matter will therefore enter, within any given portion of time, into vessels of the same capacity; and this increased quantity may frequently exceed

that which the vessels of the bark are immediately prepared to carry away. Much new wood will in consequence be generated, and increased vigour given; and, the same causes operating through successive seasons, will give the ascendancy we generally observe in the perpendicular branch.

In the preceding experiments, none of the layers, or cuttings, exceeded a few inches in length; and, to the summit of these the sap appeared to rise, through the inverted tubes of the wood, nearly as well as in those which retained their natural position. But some former experiments had induced me to suspect, that this would not be the case in longer cuttings; I therefore planted, in the autumn of 1802, twelve cuttings of the sallow, (*Salix caprea,*) inverting one half of them. The whole readily emitted roots, and grew with luxuriance; but their modes of growth were extremely different. In the cuttings which stood in their natural position, vegetation proceeded with most vigour at the points most elevated; but, in the inverted cuttings, it grew more and more languid as it became distant from the ground, and nearly ceased, towards the conclusion of the summer, at the height of four feet. The new wood also, which was generated by these inverted cuttings, accumulated above the bases of the annual shoots, as in the preceding instances.

These facts appear to prove, that the vessels of plants are not equally well calculated to carry their contents in opposite directions; and, I think, afford some grounds to suspect that the vessels of the bark, like those which constitute the venous system of animals, (to which they are in many respects analogous,) may be provided with valves, whose extreme minuteness has concealed them from observation.

The experiments, and still more, the Plates, of HALES, have

induced naturalists to draw conclusions in direct opposition to the preceding. But the Plates of that great naturalist are not always taken correctly from nature;* and Plates, under such circumstances, however fair and candid the intentions of an author may be, will too often be found somewhat better calculated to support his own hypothesis, than to elucidate the facts he intends to state.

The preceding peculiarities in the growth of inverted cuttings, appear to have escaped the observation of DU HAMEL; and, as very few instances of error, or want of accurate observation, will ever be found in the works of that excellent naturalist, I must request permission to send you some of the subjects of my experiments, as vouchers for my own accuracy.

Of the inverted cuttings employed by DU HAMEL, a small portion only appears to have remained above the ground; and, under such circumstances, the different forms of those growing in their natural, or inverted, position would be scarcely observable. It appears also, from his experiments, that such inverted cuttings, in subsequent years, grow with as much vigour as others that are not inverted; whence we must conclude, that the organization of the internal bark becomes again inverted, and adapted to the position of the branch. The growth of some inverted plants of the gooseberry-tree, which I obtained, many years ago, from layers, gave me reason to draw a different conclusion; for these always continued weak and dwarfish. I do not, however, entertain the slightest degree of doubt, but that the assertion of DU HAMEL is perfectly correct.

I intended to have added some observations on the repro-

* The eleventh Plate (Vegetable staticks) is that to which, in this place, I particularly allude.

duction of buds and roots of trees; but these would necessarily extend the present Paper to an immoderate length; I shall therefore reserve them for a future communication, and conclude with an account of an experiment which more properly belongs to the Paper I had the honour to address to you last year, but which had not then succeeded.

I have stated, in that Paper, that the leaf-stalk, the fruit-stalk, and the tendril, of the vine, had been successfully substituted, in many instances, for each other; but that I had failed in my efforts to engraft a bunch of grapes, by approach, on the leaf-stalk; owing, I conceived, to the operation having been improperly performed. In those experiments, I cut the leaf-stalk into the form of a wedge, and made an incision in the fruit-stalk, adapted to receive it; but, under such circumstances, the leaf-stalk (as I had proved by many experiments) has no power to generate new matter; and the wounds of the fruit-stalk heal so slowly, that I readily anticipated the ill success of the operation. In the last spring, I pared off similar portions of the leaf-stalk and fruit-stalk; and, bringing the wounded parts into contact, I secured them closely together, by means of a bandage, letting the leaf remain. Under these circumstances, an union took place; and the fruit-stalk being then taken off below the point of junction, and the leaf-stalk above it, the grapes drew their whole nutriment through the remaining part of the leaf-stalk. They did not, however, acquire their full size; and the seeds were small, and, I think, incapable of vegetating; but this I attribute to the want of nutriment in quantity rather than in quality; for the union of the vessels of the leaf-stalk with those of the fruit-stalk was very imperfect. The grapes, which

were the purple Frontigniac, possessed their musky flavour, in the same degree with others growing on the same plant.

There is another experiment in my last Paper, which I will also notice here, because it appears to lead to some important conclusions, and had been tried only in a single instance. I have there stated, that the stem of a young tree became elliptical, by being confined to move only in the segment of a large circle. This experiment was successfully repeated, during the last year, on other trees; but I have nothing to add to the description which I have already given.

I am, &c.

T. A. KNIGHT.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY;

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January, 1803.

1803	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Jan. 1	46	8	0	46	52	29,05	93	0,165	E	1	Cloudy.
	48	2	0	48	54	29,20	90		S	1	Fair.
2	42	8	0	42	53	29,26	93	0,345	NNE	1	Rain.
	45	2	0	45	55	29,43	90		W	1	Fair.
3	38	8	0	43	53	29,71	93		SSE	1	Cloudy.
	48	2	0	48	55	29,77	90		S	1	Cloudy.
4	38	8	0	41	53	29,69	94		E	1	Cloudy.
	45	2	0	45	54	29,61	89		E	1	Cloudy.
5	41	8	0	41	53	29,66	91		E	1	Cloudy.
	43	2	0	43	55	29,68	88		NE	1	Cloudy.
6	40	8	0	40	53	29,79	91		E	1	Cloudy.
	45	2	0	44	55	29,78	91		ESE	1	Cloudy.
7	44	8	0	44	53	29,62	93	0,215	SSE	1	Rain.
	45	2	0	45	56	29,45	93		SE	1	Rain.
8	43	8	0	43	54	29,32	93	0,400	N	1	Cloudy.
	45	2	0	45	56	29,31	88		NNE	1	Cloudy.
9	42	8	0	43	53	29,05	92	0,065	ESE	1	Cloudy.
	45	2	0	45	56	29,06	87		E	1	Cloudy.
10	34	8	0	34	52	29,12	88	0,112	E	2	Cloudy.
	35	2	0	35	53	29,21	86		E	2	Cloudy.
11	29	8	0	29	49	29,75	77		NE	2	Fair.
	31	2	0	31	52	29,88	75		NE	2	Fair.
12	24	8	0	24	47	30,00	79		NNE	1	Fine.
	32	2	0	32	50	30,00	73		NE	1	Fine.
13	22	8	0	23	45	30,03	83		NE	1	Fine.
	29	2	0	29	49	30,02	79		NE	1	Fair.
14	25	8	0	27	45	29,98	79		NE	2	Cloudy.
	30	2	0	30	48	29,92	77		NE	1	Fair.
15	28	8	0	28	46	29,88	78		NE	2	Cloudy.
	29	2	0	29	47	29,85	77		NE	2	Cloudy.
16	26	8	0	27	44	29,87	80		ENE	2	Cloudy.
	27	2	0	27	47	29,81	81		E	2	Cloudy.

METEOROLOGICAL JOURNAL
for January, 1803.

1803	Six'a Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.		Inches.	Points.	Str.	
	°					°				
Jan. 17	26	8 0	33	45	29,56	89		E	2	Cloudy.
	34	2 0	35	45	29,60	95	0,037	E	1	Foggy.
18	35	8 0	35	48	29,61	95		E	1	Cloudy.
	33	2 0	33	44	29,44	91		E	1	Cloudy.
19	36	8 0	36	47	29,45	93		E	1	Rain.
	34	2 0	34	45	29,55	93		NE	1	Cloudy.
20	37	8 0	37	48	29,61	91	0,085	NE	1	Cloudy.
	43	2 0	43	46	29,77	93		SSW	1	Cloudy.
21	42	8 0	42	49	29,72	92		SSW	1	Cloudy.
	43	2 0	43	49	29,70	95		SSE	1	Cloudy.
22	41	8 0	43	47	29,50	95		SSE	2	Cloudy.
	45	2 0	44	50	29,37	89		SSE	2	Cloudy.
23	40	8 0	41	48	29,26	93	0,120	SE	1	Cloudy.
	43	2 0	43	51	29,32	93		ESE	1	Cloudy.
24	34	8 0	35	48	29,48	92		E	1	Cloudy.
	37	2 0	37	51	29,54	88		E	1	Cloudy.
25	27	8 0	27	47	29,71	83		E	2	Cloudy.
	35	2 0	35	48	29,74	83		E	2	Cloudy.
26	19	8 0	19	45	29,70	83		NE	2	Cloudy.
	22	2 0	22	46	29,66	83		NE	2	Snow.
27	22	8 0	23	43	29,66	86		N	1	Snow.
	28	2 0	27	45	29,68	81		N	1	Fair.
28	25	8 0	28	44	29,72	84		N	1	Cloudy.
	33	2 0	32	47	29,72	81		N	1	Fair.
29	26	8 0	28	44	30,01	79		NE	2	Cloudy.
	31	2 0	30	46	30,08	79		NE	1	Fair.
30	26	8 0	27	43	30,12	84		NW	1	Fair.
	36	2 0	36	46	30,11	81		NW	1	Cloudy.
31	32	8 0	33	43	30,15	89		N	1	Cloudy.
	38	2 0	38	45	30,18	86		N	1	Cloudy.

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1803	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Feb. 1	34	7	0	34	44	30,28	90		NW	1	Cloudy.
	38	2	0	38	46	30,15	83		WNW	1	Fair.
2	35	7	0	36	46	29,96	91		W	1	Cloudy.
	41	2	0	41	48	29,77	82		W	1	Fair.
3	34	7	0	34	46	29,47	89		NW	1	Cloudy.
	37	2	0	37	48	29,61	86		NE	1	Cloudy.
4	27	7	0	27	46	30,17	79		NE	1	Fair.
	34	2	0	34	48	30,16	78		NE	1	Cloudy.
5	26	7	0	27	46	30,03	85		NE	1	Fair.
	35	2	0	35	49	29,96	80		N	1	Fair.
6	31	7	0	33	46	29,47	88		W	1	Snow.
	38	2	0	38	48	29,31	79		W	2	Cloudy.
7	29	7	0	29	46	29,40	86		NE	2	Snow.
	32	2	0	32	49	29,56	82		NE	1	Fair.
8	26	7	0	27	44	29,86	88		NE	1	Snow.
	32	2	0	32	46	29,97	81		NE	1	Cloudy.
9	28	7	0	28	44	30,17	88		NE	1	Cloudy.
	33	2	0	33	46	30,23	82		NE	1	Fair.
10	22	7	0	22	43	30,38	84		NE	1	Cloudy.
	32	2	0	32	48	30,42	77		S	1	Fair.
11	19	7	0	20	44	30,48	80		NE	1	Fair.
	32	2	0	32	45	30,46	81		NE	1	Cloudy.
12	20	7	0	20	42	30,46	87		NE	1	Cloudy.
	32	2	0	32	44	30,35	82		SSE	1	Cloudy.
13	31	7	0	36	43	29,98	94	0,145	S	1	Rain.
	43	2	0	42	44	29,78	95		S	1	Rain.
14	31	7	0	33	43	29,72	93	0,180	SW	1	Cloudy.
	43	2	0	41	45	29,67	92		SW	1	Cloudy.
15	39	7	0	40	44	29,44	94	0,052	SW	1	Cloudy.
	48	2	0	48	47	29,29	91		W	1	Cloudy.
16	40	7	0	40	47	29,27	78		W	1	Fair.
	46	2	0	45	51	29,41	74		W	1	Cloudy.

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for February, 1803.

1803	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Feb. 17	43	7	0	46	48	29,50	90		W	2	Cloudy.
	51	2	0	51	52	29.42	86		SW	2	Cloudy.
18	42	7	0	42	51	29.48	82	0,075	SW	1	Cloudy.
	47	2	0	47	52	29,55	76		SW	2	Fair.
19	36	7	0	37	50	29,74	90		SSE	1	Cloudy.
	46	2	0	46	53	29,56	90		S	1	Rain.
20	44	7	0	44	51	29,46	88		SSE	2	Cloudy.
	49	2	0	47	54	29,44	84		SSE	2	Fair.
21	40	7	0	40	53	29,60	92	0,112	NE	1	Cloudy.
	43	2	0	43	55	29.72	92		NE	1	Cloudy.
22	38	7	0	38	52	29,99	91	0,037	SW	1	Rain.
	45	2	0	45	55	30,11	73		SW	1	Fair.
23	36	7	0	36	52	30,19	88		SSE	1	Fair.
	47	2	0	47	54	30,15	79		S	1	Fair.
24	41	7	0	43	53	30,00	87		S	2	Fair.
	48	2	0	48	55	29,90	87		S	2	Cloudy.
25	36	7	0	36	53	30,00	88	0,143	W	1	Cloudy.
	48	2	0	48	55	30,02	80		SW	1	Fair.
26	46	7	0	47	54	30,01	91		SW	1	Cloudy.
	52	2	0	52	56	29,97	83		SW	1	Cloudy.
27	50	7	0	50	55	29,86	90		W	2	Cloudy.
	53	2	0	52	59	29,91	69		W	2	Fair.
28	44	7	0	44	56	30,16	83		SW	1	Cloudy.
	51	2	0	48	56	30,04	90		SW	1	Rain.

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1803	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°						°				
Mar. 1	49	7	0	49	55	29.98	88	0,016	SW	2	Cloudy.
	56	2	0	56	58	29.97	82		W	2	Cloudy.
2	49	7	0	49	56	29.77	85		W	1	Cloudy.
	53	2	0	53	58	29.60	81		W	2	Cloudy.
3	36	7	0	36	54	29.38	83	0,138	NE	2	Cloudy.
	39	2	0	39	57	29.47	76		NE	2	Fair.
4	32	7	0	33	53	29.80	85		NNE	2	Cloudy.
	40	2	0	40	57	29.88	77		NE	2	Fair.
5	28	7	0	29	53	29.95	78		W	1	Cloudy.
	37	2	0	37	55	29.94	72		SSW	1	Fair.
6	31	7	0	32	52	30.02	82		E	1	Fair.
	40	2	0	40	55	30.05	75		ESE	1	Cloudy.
7	31	7	0	31	51	30.08	81		NE	2	Cloudy.
	36	2	0	36	52	30.08	72		NE	2	Cloudy.
8	33	7	0	33	49	30.08	75		NE	2	Cloudy.
	40	2	0	40	52	30.08	70		NE	2	Hazy.
9	31	7	0	31	48	30.04	80		NE	2	Cloudy.
	36	2	0	36	51	30.07	75		NE	2	Hazy.
10	31	7	0	31	49	30.02	77		NE	2	Cloudy.
	34	2	0	34	50	30.02	76		NE	2	Cloudy.
11	31	7	0	32	48	30.12	77	0,030	NE	2	Cloudy.
	37	2	0	36	51	30.17	81		NE	2	Snow.
12	26	7	0	26	48	30.34	87		NE	2	Fair.
	36	2	0	36	52	30.39	80		NE	2	Fair.
13	25	7	0	26	48	30.38	86		W	1	Fair.
	42	2	0	40	51	30.32	76		SW	1	Fair.
14	38	7	0	40	49	30.17	92		WSW	1	Cloudy.
	52	2	0	51	53	30.08	76		W	1	Cloudy.
15	44	7	0	45	50	29.75	92		SW	1	Cloudy.
	50	2	0	50	53	29.58	92		SW	1	Rain.
16	35	7	0	36	50	29.81	87	0,132	NE	1	Cloudy.
	44	2	0	44	53	29.88	78		E	1	Fair.

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1803	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°		°				
Mar. 17	36	7	0	39	50	29,83	85		S	2	Cloudy.
	46	2	0	46	52	29,84	86		S	1	Cloudy.
18	45	7	0	45	52	30,03	82		SSW	1	Cloudy.
	54	2	0	54	55	30,07	88		S	1	Cloudy.
19	46	7	0	48	53	30,15	90		S	2	Cloudy.
	51	2	0	51	56	30,16	87		S	2	Cloudy.
20	45	7	0	46	54	30,08	89		SW	1	Cloudy.
	54	2	0	54	55	30,14	88		SSW	1	Cloudy.
21	44	7	0	44	54	30,19	93	0,200	E	1	Cloudy.
	58	2	0	58	59	30,16	84		E	1	Hazy.
22	42	7	0	42	56	30,13	94		E	1	Foggy.
	57	2	0	56	59	30,07	76		E	1	Fine.
23	44	7	0	46	57	30,08	86		E	1	Cloudy.
	61	2	0	60	60	30,12	73		S	1	Fair.
24	42	7	0	42	58	30,22	87		NE	1	Fair.
	60	2	0	60	60	30,18	75		E	1	Hazy.
25	44	7	0	45	58	30,04	84		ENE	1	Hazy.
	61	2	0	60	61	30,04	73		ESE	1	Hazy.
26	46	7	0	46	60	30,06	81		NE	1	Hazy.
	65	2	0	65	62	30,08	71		E	1	Hazy.
27	45	7	0	45	60	30,18	83		NNE	1	Fine.
	63	2	0	63	62	30,16	68		E	1	Fine.
28	42	7	0	42	60	30,08	81		E	1	Fine.
	60	2	0	59	62	30,05	68		NE	1	Hazy.
29	46	7	0	46	61	29,98	85		NE	1	Hazy.
	62	2	0	60	62	29,90	67		W	1	Hazy.
30	45	7	0	46	60	30,03	81		W	1	Hazy.
	56	2	0	56	63	30,03	68		NW	1	Fine.
31	44	7	0	44	60	30,10	80		W	1	Fair.
	66	2	0	66	64	30,12	65		SW	1	Fine.

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for April, 1803.

1803	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°		°				
April 1	43	7 0	45	61	30,05	77		SSW	1	Fine.
	65	2 0	65	63	29,91	67		S	1	Fine.
2	51	7 0	51	63	29,65	76		S	1	Cloudy.
	61	2 0	61	64	29,57	72		S	2	Fair.
3	49	7 0	50	62	29,56	81	0,050	SE	1	Cloudy.
	57	2 0	57	63	29,58	77		S	2	Cloudy.
4	46	7 0	46	61	29,72	86	0,143	SW	1	Cloudy.
	56	2 0	55	63	29,82	68		SW	2	Fair.
5	44	7 0	46	61	30,04	83		SSW	2	Fine.
	57	2 0	56	62	30,08	70		SSW	2	Cloudy.
6	43	7 0	45	60	30,16	81		WSW	1	Cloudy.
	60	2 0	60	62	30,02	67		SSE	1	Fine.
7	46	7 0	48	60	29,68	84		E	1	Cloudy.
	59	2 0	59	62	29,62	62		E	2	Fine.
8	48	7 0	48	60	29,68	87	0,065	E	1	Rain.
	52	2 0	50	61	29,75	86		W	1	Rain.
9	46	7 0	46	60	29,87	90	0,310	N	1	Rain.
	57	2 0	54	60	29,91	85		E	1	Cloudy.
10	47	7 0	50	60	30,11	88	0,040	SW	1	Cloudy.
	58	2 0	58	61	30,18	76		NW	1	Hazy.
11	46	7 0	48	60	30,37	77		NE	1	Fair.
	62	2 0	62	63	30,39	65		NE	1	Hazy.
12	42	7 0	44	60	30,50	83		NE	2	Hazy.
	57	2 0	57	62	30,47	67		E	2	Fine.
13	39	7 0	44	60	30,44	83		NE	2	Hazy.
	60	2 0	60	65	30,44	72		E	2	Fine.
14	42	7 0	45	60	30,42	81		NE	1	Hazy.
	68	2 0	68	63	30,37	61		E	1	Fine.
15	44	7 0	48	61	30,29	77		NE	1	Fine.
	67	2 0	67	64	30,23	67		E	1	Fine.
16	45	7 0	49	62	30,05	78		E	1	Hazy.
	72	2 0	70	64	29,92	61		SE	2	Fine.

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1803	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°		°				
Apr. 17	51	7	0	53	62	29,91	83		S	2	Cloudy.
	61	2	0	60	64	29,96	64		SW	2	Fair.
18	47	7	0	47	62	29,77	83	0,035	WSW	1	Rain.
	54	2	0	54	62	29,91	67		WNW	2	Fair.
19	46	7	0	47	60	29,62	87		SSW	2	Rain.
	55	2	0	55	62	29,59	70		W	2	Cloudy.
20	42	7	0	44	58	29,60	74		W	2	Cloudy.
	53	2	0	51	61	29,60	68		WNW	2	Cloudy.
21	46	7	0	52	59	29,18	87	0,068	SW	2	Cloudy.
	55	2	0	54	60	29,19	71		WNW	2	Fair.
22	41	7	0	44	58	29,31	79		SW	1	Fine.
	56	2	0	51	60	29,36	73		W	2	Cloudy.
23	40	7	0	43	57	29,56	83	0,073	SSW	2	Fair.
	52	2	0	50	59	29,58	77		WSW	1	Rain.
24	38	7	0	41	57	29,66	85	0,032	SW	1	Fine.
	56	2	0	53	59	29,70	69		SW	1	Fair.
25	38	7	0	40	57	29,93	86	0,262	W	1	Hazy.
	53	2	0	51	58	29,99	72		W	1	Cloudy.
26	36	7	0	39	56	30,01	82		S	1	Fair.
	54	2	0	54	59	29,85	67		WSW	2	Cloudy.
27	43	7	0	45	56	29,61	81		N	1	Cloudy.
	51	2	0	50	58	29,61	74		NW	1	Fair.
28	36	7	0	39	56	29,90	82	0,016	NNE	2	Fine.
	47	2	0	45	57	29,97	72		N	2	Cloudy.
29	38	7	0	41	56	30,09	78		NE	1	Cloudy.
	50	2	0	50	57	30,09	73		NW	1	Cloudy.
30	43	7	0	44	56	29,94	79		SW	1	Cloudy.
	54	2	0	53	56	29,85	71		SW	1	Cloudy.

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1808	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°		°				
May 1	46	7 0	46	55	29,60	88	0,014	SW	1	Cloudy.
	57	2 0	57	57	29,56	82		SW	1	Cloudy.
2	50	7 0	51	56	29,37	88	0,083	SSW	2	Rain. [Much wind last night.
	54	2 0	53	58	29,34	86		SSW	2	Rain.
3	40	7 0	44	56	29,55	81	0,230	SW	2	Fine.
	54	2 0	52	58	29,51	79		SSW	2	Rain.
4	42	7 0	44	56	29,53	84	0,058	SSW	1	Fair.
	56	2 0	48	58	29,53	81		WSW	1	Rain.
5	42	7 0	44	56	29,79	86	0,157	SSW	1	Fair.
	59	2 0	58	58	29,85	70		SW	1	Cloudy.
6	46	7 0	47	57	30,03	83		W	1	Fair.
	61	2 0	60	59	30,04	69		N	1	Fair.
7	45	7 0	48	57	30,04	79		NE	1	Fair.
	64	2 0	64	60	30,02	66		NNE	1	Fine.
8	43	7 0	49	58	30,16	81		NE	1	Fine.
	67	2 0	66	60	30,16	66		NE	1	Fine.
9	49	7 0	52	60	30,14	72		NE	2	Fine.
	59	2 0	57	60	30,15	67		NE	2	Cloudy.
10	49	7 0	52	60	30,00	79		NW	1	Cloudy.
	60	2 0	60	61	29,96	69		NNE	1	Cloudy.
11	48	7 0	51	60	29,95	74		NW	1	Cloudy.
	58	2 0	58	61	29,98	66		NW	1	Fair.
12	50	7 0	53	60	29,80	82		W	1	Cloudy.
	62	2 0	62	62	29,78	66		W	2	Fair.
13	42	7 0	44	60	30,04	75		NW	2	Fine.
	58	2 0	58	62	30,13	64		NW	2	Fair.
14	42	7 0	45	60	30,25	76		SW	1	Fair.
	64	2 0	63	61	30,20	64		WNW	1	Fine.
15	46	7 0	48	60	30,20	73		WNW	2	Fine.
	57	2 6	55	60	30,21	64		NNW	2	Cloudy.
16	40	7 0	43	59	30,35	73		N	1	Fine.
	58	2 0	58	60	30,25	64		NW	1	Fine.

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1803	Six's Therm least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°		°				
May 17	48	7 0	50	59	30,10	71		N	2	Cloudy.
	57	2 0	57	60	30,11	65		N	2	Cloudy.
18	45	7 0	47	59	30,28	71		N	2	Fine.
	53	2 0	53	59	30,32	68		N	2	Cloudy.
19	38	7 0	44	58	30,28	75		NNE	2	Cloudy.
	53	2 0	53	59	30,26	74		NE	2	Cloudy.
20	44	7 0	44	59	30,18	79		NNE	1	Rain.
	49	2 0	47	59	30,10	88		NE	1	Rain.
21	47	7 0	50	59	30,18	88	0,270	NE	2	Cloudy.
	58	2 0	57	61	30,18	76		NE	1	Cloudy.
22	43	7 0	46	58	30,09	82		NE	2	Cloudy.
	54	2 0	52	60	30,04	74		NE	2	Cloudy.
23	45	7 0	48	58	29,98	83		NE	2	Cloudy.
	54	2 0	54	60	29,95	78		NE	2	Cloudy.
24	47	7 0	48	58	29,94	78		NNW	1	Cloudy.
	57	2 0	56	60	29,94	66		N	1	Cloudy.
25	48	7 0	51	58	29,87	80		WSW	2	Cloudy.
	60	2 0	59	60	29,80	78		SSW	2	Cloudy.
26	51	7 0	52	59	29,67	92	0,245	NW	1	Cloudy.
	67	2 0	66	61	29,78	62		SW	1	Hazy.
27	55	7 0	57	60	29,78	88		SW	2	Cloudy.
	67	2 0	67	62	29,84	72		WSW	2	Cloudy.
28	55	7 0	56	61	29,80	92	0,210	SSW	2	Cloudy.
	69	2 0	68	62	29,86	76		SW	1	Cloudy.
29	53	7 0	56	61	29,86	83		SW	2	Cloudy.
	61	2 0	61	62	29,78	78		SSW	2	Cloudy.
30	55	7 0	57	61	29,77	85	0,065	SW	2	Cloudy.
	66	2 0	66	62	29,77	73		SW	2	Fair.
31	53	7 0	55	61	29,97	88	0,353	WSW	2	Cloudy.
	67	2 0	67	62	30,01	69		SSW	2	Fair.

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for June, 1803.

1803	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.	°	Inches.			
June 1	49	7	0	52	61	30,16	87	0,095	W	2	Fine.
	67	2	0	67	62	30,16	70		SW	2	Fair.
2	54	7	0	56	61	29,84	77		ESE	2	Cloudy.
	61	2	0	60	62	29,72	85		S	2	Rain.
3	50	7	0	53	61	29,74	83	0,285	S	2	Fair.
	62	2	0	60	62	29,69	79		S	2	Cloudy.
4	52	7	0	54	61	29,68	83	0,123	SSE	2	Fair.
	65	2	0	65	62	29,63	78		S	2	Cloudy.
5	53	7	0	55	62	29,63	83		E	1	Cloudy.
	61	2	0	61	62	29,64	80		N	1	Rain.
6	51	7	0	53	61	29,68	84	0,200	SW	1	Fair.
	65	2	0	65	62	29,70	74		SSW	1	Fair. [Lightning with thund.
7	51	7	0	53	61	29,85	83	0,320	WSW	2	Cloudy.
	61	2	0	59	61	29,90	70		WSW	2	Cloudy.
8	50	7	0	50	61	29,80	84	0,040	E	1	Rain.
	54	2	0	54	61	29,71	91		NE	1	Rain.
9	50	7	0	53	60	29,87	90	0,318	E	1	Cloudy.
	61	2	0	60	61	29,75	86		E	1	Rain. [Lightning with thunder.
10	52	7	0	53	60	29,81	80	0,661	SW	2	Fair.
	63	2	0	59	61	29,94	73		SW	2	Rain.
11	49	7	0	53	60	30,02	83	0,118	SSW	2	Cloudy.
	64	2	0	63	61	30,02	70		SSW	1	Cloudy.
12	57	7	0	58	60	30,00	90		S	1	Cloudy.
	66	2	0	66	61	29,96	82		S	1	Cloudy.
13	58	7	0	59	61	29,91	87	0,230	SW	1	Cloudy.
	67	2	0	66	62	29,89	77		SW	1	Cloudy.
14	58	7	0	58	61	30,02	75		WSW	1	Fair.
	73	2	0	73	64	30,04	67		SW	1	Fair.
15	53	7	0	57	62	30,04	79		SW	1	Fair.
	66	2	0	63	63	29,99	80		SW	2	Rain.
16	56	7	0	58	62	30,18	83	0,016	WSW	1	Cloudy.
	74	2	0	73	64	30,20	67		WSW	1	Fair.

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1803	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	°			°	°	Inches.	°	Inches.	Points.	Str.	
June 17	58	7	0	59	63	30,14	78		W	2	Cloudy.
	71	2	0	71	65	30,17	68		W	2	Fair.
18	58	7	0	60	64	29,95	92	0,280	W	1	Rain.
	69	2	0	67	65	29,95	80		W	1	Cloudy.
19	57	7	0	57	64	29,95	85	0,097	WSW	1	Rain.
	61	2	0	60	64	29,84	84		S	1	Rain.
20	54	7	0	56	63	29,86	78	0,088	W	1	Cloudy.
	65	2	0	59	63	29,82	76		W	1	Rain.
21	50	7	0	52	62	29,93	82	0,460	NNW	2	Fine.
	64	2	0	64	64	30,12	61		NW	2	Fine.
22	49	7	0	55	62	30,19	77		WSW	1	Cloudy.
	59	2	0	58	62	30,08	78		SSW	1	Rain.
23	47	7	0	51	61	30,23	80	0,028	WNW	1	Fair.
	65	2	0	64	62	30,25	67		NW	1	Cloudy.
24	51	7	0	54	62	30,30	78		NNW	1	Fine.
	69	2	0	68	63	30,30	68		NNE	1	Hazy.
25	51	7	0	55	62	30,38	77		ENE	1	Hazy.
	68	2	0	67	63	30,38	69		E	1	Fair.
26	52	7	0	56	62	30,46	78		ENE	1	Cloudy.
	68	2	0	68	63	30,45	66		NE	1	Cloudy.
27	51	7	0	55	62	30,41	78		ENE	1	Cloudy.
	68	2	0	67	63	30,34	70		NE	1	Cloudy.
28	53	7	0	54	62	30,28	85		NE	1	Cloudy.
	70	2	0	70	64	30,23	71		E	1	Fine.
29	53	7	0	56	63	30,23	83		E	1	Cloudy.
	71	2	0	71	65	30,21	73		NE	1	Fair.
30	51	7	0	56	64	30,22	81		NE	1	Cloudy.
	67	2	0	67	65	30,23	70		NE	1	Fair.

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1803	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.	°	Inches.	Points.	Str.	
July 1	52	7 0	54	64	30,18	84		ENE	1	Cloudy.
	76	2 0	74	66	30,13	71		NE	1	Fine.
2	59	7 0	63	65	30,00	81		NE	1	Fine.
	86	2 0	85	68	29,96	64		ESE	1	Hazy.
3	65	7 0	67	67	29,99	72	0,130	N	1	Fair.
	80	2 0	79	70	29,99	63		NW	1	Fine.
4	62	7 0	64	69	29,99	79		SW	1	Fine.
	81	2 0	80	70	29,96	62		SSW	2	Fine.
5	61	7 0	61	69	29,99	81		NW	1	Cloudy.
	71	2 0	70	69	29,97	68		SW	1	Cloudy.
6	52	7 0	57	68	29,97	75	0,030	NW	1	Fair.
	66	2 0	65	68	30,02	68		NW	1	Fair.
7	51	7 0	55	67	30,13	75	0,060	SW	2	Cloudy.
	68	2 0	67	67	30,07	69		S	2	Cloudy.
8	60	7 0	61	66	30,01	78	0,022	SSE	1	Fair.
	76	2 0	75	68	29,96	62		SSE	1	Fine.
9	59	7 0	62	68	29,98	84	0,135	WNW	1	Cloudy.
	75	2 0	73	68	30,05	62		WNW	1	Fine.
10	55	7 0	59	68	30,35	71		NW	1	Fine.
	73	2 0	71	68	30,38	62		N	1	Fine.
11	58	7 0	61	68	30,45	72		E	1	Fine.
	75	2 0	75	69	30,40	64		E	1	Fine.
12	57	7 0	62	69	30,28	73		E	1	Fair.
	79	2 0	79	70	30,18	66		ESE	1	Cloudy.
13	63	7 0	66	68	30,13	72		N	1	Fair.
	71	2 0	70	71	30,17	64		NW	1	Fine.
14	57	7 0	61	70	30,23	74		NE	2	Fine.
	65	2 0	63	67	30,27	68		ENE	2	Cloudy.
15	56	7 0	56	68	30,31	78		NE	2	Cloudy.
	64	2 0	64	68	30,28	70		NE	2	Cloudy.
16	56	7 0	57	67	30,22	77		NE	2	Cloudy.
	69	2 0	69	68	30,22	71		NE	2	Fair.

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1803	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
July 17	55	7	0	56	66	30,22	81		NE	1	Cloudy.
	75	2	0	71	68	30,20	74		NE	1	Fair.
18	58	7	0	58	67	30,22	81		ENE	1	Cloudy.
	77	2	0	77	70	30,18	66		E	2	Fine.
19	59	7	0	64	68	30,14	76		E	2	Fair.
	79	2	0	79	71	30,08	67		E	2	Fine.
20	61	7	0	64	69	30,02	76		E	1	Fair.
	80	2	0	80	73	29,97	65		E	1	Fine.
21	56	7	0	62	70	30,07	91	0,395	NNE	1	Cloudy.
	71	2	0	71	70	30,14	75		NE	1	Fair.
22	58	7	0	60	69	30,29	83		E	1	Cloudy.
	78	2	0	76	70	30,28	64		SE	1	Fine.
23	59	7	0	63	70	30,28	77		S	1	Fair.
	82	2	0	80	71	30,21	63		WSW	1	Fine.
24	59	7	0	60	70	30,18	83		NE	1	Rain.
	71	2	0	70	71	30,17	67		NE	1	Cloudy.
25	51	7	0	58	69	30,19	77	0,055	NE	1	Fine.
	68	2	0	66	69	30,17	63		NE	1	Fair.
26	54	7	0	56	68	30,15	74		ENE	1	Fine.
	76	2	0	75	69	30,14	62		NW	1	Fair.
27	57	7	0	61	69	30,14	76		SW	1	Fair.
	75	2	0	74	69	30,10	65		E	1	Fair.
28	55	7	0	58	69	30,04	80		NE	1	Fair.
	76	2	0	76	70	29,97	67		E	1	Fine.
29	59	7	0	64	69	29,93	77		ENE	1	Hazy.
	75	2	0	75	69	30,01	85		S	1	Rain.
30	65	7	0	67	70	30,07	91	0,263	S	1	Cloudy.
	76	2	0	74	70	30,09	78		S	1	Fair.
31	66	7	0	67	70	30,06	84	0,278	S	1	Fine.
	81	2	0	81	73	29,98	67		S	1	Fine.

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1803	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Aug. 1	64	7	0	65	72	29,92	80		SW	1	Fine.
	77	2	0	77	72	29,88	64		SSW	2	Fair.
2	60	7	0	62	69	29,95	81		SSW	2	Fair.
	73	2	0	73	70	29,89	67		SSW	2	Fair.
3	60	7	0	61	70	29,83	78		SSW	2	Fair.
	72	2	0	72	70	29,81	65		SSW	2	Fair.
4	53	7	0	56	68	30,10	76		SW	1	Fair.
	72	2	0	72	70	30,14	60		SW	1	Fine.
5	57	7	0	59	68	30,16	76		S	1	Fine.
	78	2	0	78	71	30,12	58		SSW	1	Fine.
6	62	7	0	63	68	29,99	83		SSW	2	Cloudy.
	76	2	0	75	71	29,89	69		SSW	2	Cloudy.
7	56	7	0	58	68	29,98	74		W	1	Fair.
	71	2	0	71	68	29,98	63		WNW	1	Cloudy.
8	56	7	0	57	67	30,04	74		NE	1	Fine.
	74	2	0	71	68	30,01	67		S	1	Fair.
9	58	7	0	60	67	29,98	76		S	1	Cloudy.
	75	2	0	74	69	29,92	68		S	1	Cloudy.
10	62	7	0	62	68	29,78	81	0,033	SSW	1	Cloudy.
	73	2	0	72	69	29,74	62		W	1	Fair.
11	55	7	0	59	68	29,92	80		WSW	1	Fair.
	70	2	0	70	68	29,98	67		WNW	1	Cloudy.
12	62	7	0	63	68	30,05	84		W	1	Cloudy.
	78	2	0	77	69	30,05	67		WNW	1	Fair.
13	60	7	0	62	68	30,09	82		SW	1	Fair.
	78	2	0	78	69	30,09	64		W	1	Fair.
14	62	7	0	63	68	30,19	81		NE	1	Fine.
	77	2	0	77	71	30,19	65		E	1	Fine.
15	61	7	0	63	69	30,20	83		E	1	Cloudy.
	76	2	0	75	71	30,16	68		NE	1	Fine.
16	60	7	0	64	71	30,13	88		NE	1	Hazy.
	81	2	0	80	73	30,08	58		E	2	Fine.

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		H. M.			Inches.		Inches.	Points.	Str.	
	°					°				
Aug. 17	63	7 0	65	71	30,09	82		NE	1	Fine.
	79	2 0	78	74	30,09	63		E	1	Fine.
18	61	7 0	64	71	30,09	80		NE	1	Hazy.
	79	2 0	79	74	30,04	61		NE	1	Fine.
19	60	7 0	60	70	29,98	69		NW	2	Cloudy.
	68	2 0	68	72	29,99	65		NW	1	Cloudy.
20	52	7 0	53	68	30,01	73		WNW	1	Cloudy.
	66	2 0	66	70	30,01	67		NNW	1	Cloudy.
21	49	7 0	51	65	30,11	70		NE	1	Fine.
	65	2 0	62	68	30,11	62		N	2	Fair.
22	52	7 0	54	65	30,13	69		NW	1	Fair.
	67	2 0	67	68	30,14	64		WNW	1	Fair.
23	50	7 0	52	63	30,18	67		WNW	1	Fine.
	71	2 0	71	68	30,13	63		WNW	1	Fair.
24	53	7 0	57	64	30,06	78		W	1	Cloudy.
	74	2 0	73	67	30,04	62		W	1	Fine.
25	54	7 0	56	67	30,03	82		SW	2	Fine.
	76	2 0	76	69	29,98	66		SW	2	Fine.
26	55	7 0	57	68	30,16	72		NE	2	Fair.
	66	2 0	66	69	30,21	63		NE	2	Fine.
27	47	7 0	52	67	30,31	73		NE	2	Fine.
	67	2 0	67	69	30,29	62		NE	1	Fine.
28	47	7 0	53	67	30,21	72		N	1	Fine.
	74	2 0	73	68	30,17	60		SW	1	Fine.
29	56	7 0	58	68	30,16	82		SSW	1	Fine.
	75	2 0	74	70	30,08	62		SSW	1	Fine.
30	56	7 0	58	66	30,00	83	0,080	SW	1	Rain.
	60	2 0	57	67	30,02	93		NNE	1	Rain.
31	49	7 0	51	65	30,17	84	0,675	NNE	1	Fine.
	64	2 0	64	67	30,21	62		N	1	Fair.

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		H. M.			Inches.		Inches.	Points.	Str.	
	°					°				
Sept. 1	48	7 0	49	63	30,28	80		NW	1	Cloudy.
	63	2 0	63	65	30,28	67		NW	1	Cloudy.
2	49	7 0	51	64	30,28	80		W	1	Cloudy.
	69	2 0	69	65	30,24	71		W	1	Fair.
3	55	7 0	57	65	30,24	88		W	1	Fair.
	73	2 0	73	67	30,17	68		NW	1	Cloudy.
4	49	7 0	51	61	30,34	73		NE	2	Fine.
	60	2 0	60	64	30,36	62		NE	2	Fair.
5	43	7 0	44	63	30,42	73		WNW	1	Fine.
	64	2 0	63	65	30,38	62		W	1	Fine.
6	48	7 0	49	64	30,38	80		W	1	Fine.
	68	2 0	68	66	30,38	62		WSW	1	Fine.
7	52	7 0	52	64	30,39	78		WSW	1	Fine.
	70	2 0	69	65	30,33	67		W	1	Hazy.
8	53	7 0	54	64	30,26	76		S	1	Fine.
	75	2 0	74	67	30,22	65		SSW	1	Fine.
9	52	7 0	52	64	30,22	78		SW	1	Fine.
	73	2 0	72	67	30,22	67		NW	1	Fine.
10	50	7 0	51	65	30,33	75		WSW	1	Fine.
	66	2 0	65	66	30,31	68		W	1	Fine.
11	52	7 0	53	65	30,10	78		WSW	1	Fine.
	72	2 0	72	67	30,10	67		N	1	Fair.
12	45	7 0	46	65	30,33	73		NE	1	Fine.
	59	2 0	57	65	30,35	64		NE	2	Cloudy.
13	42	7 0	43	63	30.38	75		NE	2	Fine.
	60	2 0	59	63	30,34	64		NE	2	Fair.
14	45	7 0	46	62	30,32	72		NE	1	Fair.
	61	2 0	61	64	30,25	67		E	1	Fine.
15	45	7 0	46	62	30,12	78		NE	1	Fine.
	65	2 0	65	64	30,04	63		E	1	Fair.
16	46	7 0	47	62	29,92	73		SSW	1	Fine.
	66	2 0	65	64	29,86	64		WSW	1	Fine.

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	°			°	°	Inches.	°	Inches.			
Sep. 17	52	7	0	53	62	29,66	80	0,052	W	1	Fine.
	60	2	0	59	63	29,71	67		WNW	1	Cloudy.
18	45	7	0	47	61	29,78	73		SW	1	Cloudy.
	61	2	0	61	62	29,76	63		S	1	Fair.
19	48	7	0	50	60	29,85	79		SSW	1	Cloudy.
	60	2	0	60	61	29,83	84		S	1	Cloudy.
20	53	7	0	54	60	29,60	85	0,235	SE	1	Fine.
	58	2	0	55	61	29,38	90		S	1	Rain.
21	45	7	0	47	59	29,25	85	0,632	SW	2	Rain.
	58	2	0	57	60	29,62	73		NW	2	Cloudy.
22	40	7	0	42	58	29,97	80		W	1	Fine.
	56	2	0	56	58	30,04	69		NW	1	Cloudy.
23	44	7	0	46	58	30,09	79		W	1	Cloudy.
	56	2	0	56	58	30,11	69		NW	1	Cloudy.
24	38	7	0	40	57	30,27	79		W	1	Fine.
	56	2	0	56	58	30,32	72		NE	1	Fair.
25	39	7	0	41	56	30,37	81		NE	1	Fair.
	62	2	0	62	59	30,37	73		S	1	Fine.
26	43	7	0	43	57	30,39	83		SSW	1	Fair.
	62	2	0	62	61	30,36	71		SSE	1	Fine.
27	43	7	0	45	60	30,32	84		E	1	Fair.
	63	2	0	63	64	30,26	73		E	1	Fine.
28	48	7	0	48	60	30,20	92		NE	1	Fine.
	62	2	0	62	62	30,11	68		E	1	Fine.
29	46	7	0	46	60	29,94	87		NE	1	Fine.
	66	2	0	66	64	29,84	70		E	1	Fine.
30	48	7	0	48	61	29,75	89		E	1	Cloudy.
	59	2	0	59	62	29,70	86		E	1	Rain.

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		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°		°				
Oct. 1	44	7 0	44	61	29,87	82	0,063	N	1	Fine.
	56	2 0	56	63	29,91	67		NNW	1	Fine.
2	40	7 0	41	60	29,97	83		WNW	1	Fine.
	54	2 0	54	61	29,96	63		NW	1	Cloudy.
3	42	7 0	43	59	29,78	85		WSW	1	Cloudy.
	50	2 0	50	60	29,73	73		E	1	Cloudy.
4	38	7 0	39	58	29,77	84		WSW	1	Cloudy.
	52	2 0	50	60	29,82	81		WSW	1	Rain.
5	38	7 0	39	58	29,97	87	0,115	W	1	Fine.
	55	2 0	54	60	30,02	72		WNW	1	Fair.
6	48	7 0	48	58	30,16	85		N	1	Cloudy.
	55	2 0	55	60	30,21	80		N	1	Cloudy.
7	45	7 0	45	59	30,20	80		NNE	1	Cloudy.
	54	2 0	54	61	30,05	69		WNW	1	Fine.
8	39	7 0	39	58	29,78	79	0,063	NNW	1	Fine.
	50	2 0	50	61	29,68	81		N	2	Fair.
9	46	7 0	46	58	29,86	83	0,060	NE	2	Cloudy.
	51	2 0	51	60	29,80	76		NE	2	Fair.
10	45	7 0	45	57	29,82	78		NNE	2	Fine.
	53	2 0	53	59	29,88	72		NNE	2	Cloudy.
11	42	7 0	43	57	30,05	80		WNW	1	Cloudy.
	53	2 0	53	59	30,10	70		NW	1	Cloudy.
12	48	7 0	49	57	30,11	80		W	1	Cloudy.
	57	2 0	57	59	30,14	73		WSW	1	Cloudy.
13	50	7 0	50	58	30,12	80		WSW	1	Cloudy.
	59	2 0	59	60	30,06	73		SSW	1	Cloudy.
14	47	7 0	48	58	29,83	78		SSE	1	Fair.
	55	2 0	53	59	29,72	81		SE	1	Rain.
15	43	7 0	43	58	29,73	90	0,173	WSW	1	Cloudy.
	60	2 0	60	61	29,72	74		SW	1	Cloudy.
16	50	7 0	51	59	29,81	91		SSW	1	Cloudy.
	63	2 0	63	62	29,80	75		SSW	1	Fair.

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1803	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Oct. 17	49	7	0	49	60	29,82	80		W	1	Fine.
	59	2	0	58	63	29,91	65		WNW	1	Fine.
18	51	7	0	55	60	29,96	87		WSW	2	Cloudy.
	65	2	0	65	62	30,04	79		SW	1	Cloudy.
19	53	7	0	56	62	30,20	86		SW	1	Cloudy.
	64	2	0	64	64	30,21	74		SW	1	Fine.
20	55	7	0	55	62	30,13	82		SW	2	Cloudy.
	62	2	0	64	63	30,18	78		WNW	1	Cloudy.
21	58	7	0	58	63	30,25	88		WSW	1	Cloudy.
	67	2	0	67	64	30,26	77		NW	1	Cloudy.
22	58	7	0	58	64	30,27	84		W	1	Cloudy.
	62	2	0	60	65	30,30	73		N	1	Cloudy.
23	45	7	0	46	63	30,38	86		WNW	1	Cloudy.
	58	2	0	58	65	30,41	74		NW	1	Fair.
24	43	7	0	43	62	30,46	83		NE	1	Foggy.
	58	2	0	58	65	30,46	75		E	1	Fine.
25	42	7	0	46	62	30,45	82		E	1	Cloudy.
	55	2	0	55	63	30,44	81		E	1	Cloudy.
26	46	7	0	48	62	30,48	83		E	1	Cloudy.
	50	2	0	50	62	30,47	81		E	1	Cloudy.
27	49	7	0	49	61	30,39	80		E	1	Cloudy.
	56	2	0	56	64	30,34	76		E	1	Fine.
28	39	7	0	40	60	30,28	83		NE	1	Foggy.
	54	2	0	54	63	30,22	77		E	1	Fine.
29	43	7	0	43	59	30,09	90		NE	1	Foggy.
	51	2	0	48	61	30,07	86		NE	1	Foggy.
30	43	7	0	46	59	30,07	87		NE	1	Cloudy.
	51	2	0	51	60	30,04	80		NE	1	Cloudy.
31	45	7	0	46	58	30,05	86		NE	1	Cloudy.
	51	2	0	50	59	30,08	76		NE	2	Cloudy.

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1803	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Nov. 1	44	7	0	44	58	30,18	72		NE	2	Cloudy.
	47	2	0	46	59	30,20	70		E	2	Cloudy.
2	40	7	0	41	56	30,20	70		E	2	Cloudy.
	45	2	0	44	59	30,23	68		NE	2	Fair.
3	34	7	0	35	54	30,24	76		NE	2	Cloudy.
	44	2	0	44	58	30,27	70		NE	2	Fine.
4	32	7	0	34	54	30,11	77		NE	2	Fine.
	43	2	0	43	58	29,98	72		NE	2	Fine.
5	35	7	0	37	54	29,68	73		E	2	Cloudy.
	39	2	0	39	55	29,51	73		E	2	Cloudy.
6	36	7	0	39	52	29,24	86		NE	1	Cloudy.
	51	2	0	48	55	29,15	91		ESE	1	Rain.
7	44	7	0	45	53	29,42	90	0,048	S	1	Fair.
	55	2	0	54	55	29,44	81		SE	2	Fair.
8	49	7	0	50	54	29,31	91	0,112	ESE	1	Rain.
	57	2	0	56	58	29,35	83		S	1	Fair.
9	49	7	0	49	55	29,10	93	0,332	SE	1	Rain.
	51	2	0	51	56	28,98	92		SSE	2	Rain.
10	46	7	0	49	56	28,69	93	0,141	S	2	Rain. [Much wind last night.
	54	2	0	52	58	28,71	82		S	2	Cloudy.
11	44	7	0	45	57	28,75	91	0,230	S	2	Rain.
	49	2	0	49	58	28,67	90		S	2	Rain.
12	43	7	0	44	57	28,76	85	0,266	SSW	2	Rain.
	51	2	0	50	58	28,91	81		SW	1	Fair.
13	43	7	0	43	56	29,18	87		WSW	1	Fine.
	51	2	0	50	58	29,30	81		W	1	Fair.
14	42	7	0	42	56	29,56	87	0,030	WSW	1	Cloudy.
	47	2	0	47	58	29,58	81		SW	1	Fair.
15	32	7	0	34	55	29,70	87		NE	1	Cloudy.
	49	2	0	49	57	29,68	83		SSE	1	Cloudy.
16	43	7	0	44	55	29,40	92	0,148	NE	1	Cloudy.
	53	2	0	53	56	29,18	90		S	2	Cloudy.

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1803	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Nov. 17	40	7	0	40	55	29,20	90	0,070	SW	1	Cloudy.
	45	2	0	45	57	29,33	78		WNW	1	Fine.
18	35	7	0	35	55	29,28	88		W	1	Fine.
	43	2	0	43	57	29,39	85		SW	1	Fine.
19	37	7	0	38	54	29,25	89		NW	1	Cloudy.
	45	2	0	45	57	29,33	84		NE	1	Fair.
20	38	7	0	38	54	29,50	84		NE	1	Cloudy.
	46	2	0	46	56	29,44	86		NE	1	Cloudy.
21	42	7	0	43	53	29,07	94	0,275	NE	1	Rain.
	50	2	0	49	57	29,06	96		NW	1	Rain.
22	40	7	0	48	54	29,07	92	0,202	S	2	Rain. [Much wind last night.
	50	2	0	50	56	29,04	87		S	2	Rain.
23	41	7	0	42	54	29,15	89	0,405	WSW	2	Cloudy.
	50	2	0	49	56	29,34	81		W	2	Fair.
24	34	7	0	34	53	29,61	87		W	2	Fair.
	43	2	0	43	54	29,69	79		W	1	Cloudy.
25	35	7	0	35	52	29,58	84	0,042	WNW	2	Fair.
	41	2	0	41	53	29,68	80		WNW	2	Fair.
26	36	7	0	36	52	29,88	83		NW	2	Fine.
	45	2	0	45	54	29,98	81		WNW	2	Cloudy.
27	31	7	0	31	52	30,07	90		SW	1	Foggy.
	41	2	0	41	53	30,08	90		SW	1	Cloudy.
28	40	7	0	44	52	29,94	94		SW	2	Cloudy.
	51	2	0	51	54	29,88	90		SW	2	Cloudy.
29	47	7	0	47	52	29,68	95		SW	1	Cloudy.
	53	2	0	51	55	29,50	90		NW	1	Rain.
30	39	7	0	39	52	30,15	87	0,140	NE	1	Fair.
	42	2	0	41	56	30,33	86		NE	1	Fine.

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1809	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°		°				
Dec. 1	35	8 0	36	53	30,53	78		NW	1	Cloudy.
	48	2 0	48	54	30,45	87		SW	1	Cloudy.
2	45	8 0	46	53	30,13	93	0,031	SW	1	Cloudy.
	51	2 0	51	54	30,00	86		SW	1	Cloudy.
3	46	8 0	46	53	29,72	83		WNW	1	Cloudy.
	47	2 0	47	56	29,56	75		NW	1	Fair.
4	34	8 0	34	53	29,51	83		N	1	Cloudy.
	38	2 0	37	55	29,51	79		NNE	1	Fair.
5	28	8 0	31	51	29,43	82		NE	1	Cloudy.
	35	2 0	35	53	29,42	84		NE	1	Cloudy.
6	31	8 0	32	49	29,50	94		NE	1	Snow.
	35	2 0	34	51	29,54	89		NE	1	Fair.
7	30	8 0	31	48	29,72	92		NE	1	Cloudy.
	32	2 0	32	51	29,78	92		NE	1	Cloudy.
8	29	8 0	29	48	30,00	93		NE	1	Cloudy.
	33	2 0	32	50	30,08	89		NW	1	Fair.
9	21	8 0	21	46	30,18	90		W	1	Fine.
	33	2 0	32	50	30,16	89		W	1	Fair.
10	25	8 0	30	46	29,93	92		S	1	Cloudy.
	39	2 0	37	47	29,70	92		SE	1	Rain.
11	36	8 0	38	46	29,48	95	0,160	S	1	Fair.
	43	2 0	43	48	29,44	93		S	1	Cloudy.
12	36	8 0	38	47	29,37	95				Foggy.
	41	2 0	41	49	29,40	94		WNW	1	Foggy.
13	33	8 0	36	46	29,58	93	0,025	WSW	1	Rain.
	40	2 0	40	48	29,56	93		SW	1	Cloudy.
14	36	8 0	37	47	29,57	95				Foggy.
	41	2 0	41	49	29,54	95		E	1	Foggy.
15	37	8 0	37	47	29,27	94	0,137	E	1	Rain.
	40	2 0	40	50	29,17	94		E	1	Rain.
16	40	8 0	43	49	29,15	96	0,165			Foggy.
	48	2 0	48	52	29,13	97		E	1	Cloudy.

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1809	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.		Inches.	Points.	Str.	
	°					°				
Dec. 17	45	8 0	45	52	29.38	98		NE	1	Cloudy.
	46	2 0	45	53	29.45	98		E	1	Rain.
18	44	8 0	45	51	29.48	98	0,125	E	1	Cloudy.
	48	2 0	47	53	29.47	98		E	1	Cloudy.
19	45	8 0	47	52	29.47	98	0,147	E	1	Foggy.
	50	2 0	50	53	29.50	98		E	1	Rain.
20	47	8 0	47	52	29.07	99	0,170	E	1	Foggy.
	48	2 0	48	55	29.70	99		E	1	Foggy.
21	47	8 0	48	53	29.72	99	0,048	ESE	1	Cloudy.
	52	2 0	52	54	29.70	97		SE	1	Cloudy.
22	49	8 0	49	55	29.82	97	0,025	SE	1	Cloudy.
	51	2 0	51	57	29.82	96		S	1	Cloudy.
23	44	8 0	44	55	29.75	91	0,075	SW	1	Fine.
	50	2 0	50	58	29.91	85		W	1	Fair.
24	47	8 0	55	56	29.70	97	0,360	S	2	Rain.
	55	2 0	55	58	29.64	95		S	2	Cloudy.
25	50	8 0	53	56	29.59	93	0,020	S	2	Rain. [Much wind last night.
	54	2 0	52	58	29.30	80		S	2	Cloudy.
26	48	8 0	52	56	29.62	92	0,022	S	2	Rain. [Much wind last night.
	53	2 0	52	58	29.64	87		S	1	Fair.
27	47	8 0	49	57	29.61	73		S	2	Cloudy.
	53	2 0	53	58	29.35	87		SE	2	Rain.
28	47	8 0	47	56	28,98	93	0,355	ENE	1	Rain.
	49	2 0	48	58	29.16	93		W	1	Fair.
29	48	8 0	46	56	29.58	92	0,625	SSW	1	Cloudy.
	51	2 0	50	57	29.48	92		S	2	Rain.
30	49	8 0	53	56	29.32	96	0,260	S	2	Rain.
	55	2 0	55	58	29.29	96		S	2	Cloudy.
31	47	8 0	47	56	29.43	94	0,340	W	1	Cloudy.
	48	2 0	48	58	29.53	94		W	1	Rain.

1803.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	48	19	35,3	48	19	35,7	56	43	49,0	30,18	29,05	29,65	95	73	86,9	1,544
February	53	19	38,3	52	20	38,5	59	42	49,1	30,48	29,27	29,86	95	69	85,1	0,744
March	66	25	44,4	66	26	44,5	64	48	55,1	30,39	29,38	30,03	94	65	80,5	0,449
April	72	36	50,4	70	39	51,0	65	56	60,3	30,50	29,18	29,89	90	61	76,2	1,094
May	69	38	53,0	68	44	53,8	62	55	59,4	30,35	29,34	29,95	92	64	76,3	1,685
June	74	49	59,0	73	50	59,8	65	60	62,2	30,46	29,63	30,02	91	61	78,2	3,339
July	86	51	66,3	85	54	67,2	73	64	68,8	30,45	29,93	30,13	91	62	72,7	1,368
August	81	47	64,6	80	51	65,4	74	63	68,7	30,31	29,81	30,06	93	58	71,3	0,755
September	75	38	55,1	74	40	55,5	67	56	62,4	30,42	29,25	30,12	92	62	74,3	0,919
October	67	38	51,1	67	39	51,3	65	57	60,6	30,48	29,72	30,07	91	63	79,4	0,474
November	57	31	43,7	56	31	44,0	59	52	55,3	30,33	28,67	29,50	96	68	84,8	2,441
December	55	21	44,7	56	21	43,3	58	46	52,6	30,53	28,98	29,61	99	75	92,1	3,090
Whole year			50,5			50,9			58,6			29,91			79,8	17,922

* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.